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I.—HR. VON WRIGHT ON THE LOGIC OF INDUCTION (III.).

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THEOREMS CONNECTING PROBABILITY WITH INDUCTION (*Continued*).

Theorem 4.   *The Universal Generalisation Theorem.*—We have seen that it is necessary, in the case of infinite classes, to draw a distinction between the Extreme Statistical Generalisation, ' The proportion of R's in an indefinitely extended sequence of Q's is 100 % ', and the Universal Generalisation, ' All the Q's in a certain indefinitely extended sequence will be R's '. In our notation the former proposition is symbolised by $f_\infty(R ; Q) = 1$. The latter might be symbolised by $U(R ; Q)$.

We want to evaluate the probability

$$U(R ; Q)/ : k \,.\, \& \,.\, f_N(R ; Q) = 1,$$

and to see what happens to it when N is indefinitely increased. For shortness I shall write U for $U(R ; Q)$, and, as before, $P_N(1)$ for $f_N(R ; Q) = 1$.

There are two theorems to be proved, which we will call (4·1) and (4·2). The first states that, under certain conditions, $U/k \,\&\, P_N(1)$ increases with every increase of N. The second states that, under certain conditions, $U/k \,\&\, P_N(1)$ approaches to 1 as its limit if N increased indefinitely.

(4·1) By using the Lemmas it is very easy to show, as Keynes does in his *Treatise on Probability*, that

$$U/k \,\&\, P_{N+1}(1) = \,.\, \frac{U/k \,\&\, P_N(1)}{R(x_{N+1})/k \,\&\, P_N(1)}.$$

13

Hence it is plain that $U/k \& P_{N+1}(1)$ is greater than $U/k \& P_N(1)$ provided that (i) $U/k \& P_N(1)$ is not zero, and (ii) $R(x_{N+1})/k\&P_N(1)$ is less than 1.

The following is the meaning of the second condition. It must not be the case that the fact that the first N trials of Q's have all been R's makes it *certain* that the next Q tried will be an R. This condition can be granted without hesitation. Keynes, in a very obscure argument which uses the Identity of Indiscernibles as one of its premisses, draws from this condition the conclusion that an increase in the number of instances favourable to a universal generalisation strengthens the latter *only* in so far as it increases the negative analogy. Hr. von Wright attempts to state this argument of Keynes's clearly and to refute it. I have never been able to see any force in the argument myself, and I do not propose to linger over the refutation of it.

(4·2) It is also easy to show, as Keynes does in his *Treatise*, by means of the Lemmas that

$$U/k \& P_N(1) = \frac{U/k}{U/k + \overline{U}/k \times P_N(1)/k \& \overline{U}}.$$

The necessary and sufficient conditions for this expression to approach 1 as its limit when N is indefinitely increased are the following.

(i) U/k is not zero.

(ii) As N tends to infinity $P_N(1)/k \& \overline{U}$ tends to 0.

Hr. von Wright has no difficulty in showing that the second of these conditions cannot be granted. The supposal that U is false would be satisfied if even a single instance of Q turned out not to be R. But, as we have seen, the occurrence of any finite number of such counter-instances in an indefinitely long series of trials would not suffice to reduce to 0 the probability that the proportion of Q's which are R tends to the limit of 100 % as the sequence of trials is indefinitely prolonged.

The point can be made perfectly plain by the following examples of drawing counters from a bag. We have to compare the following two cases. (i) The bag used for the experiment may contain a large or a small number of counters, but in either case there is literally *no* non-white counter among them. (ii) The bag contains an enormously great number of counters and among them are a very few non-whites. In the first case the universal generalisation, 'All drawings will be white', is true from the nature of the bag. In the second case there is always a possibility at each drawing that the counter drawn

will not be white, no matter how small the probability may be. The probability that there will be *at least one* non-white drawing in the first N drawings is in fact $1 - \left(1 - \dfrac{m}{n}\right)^{N}$, if n be the number of counters in the bag and m be the number of these which are not white. In this case, then, the universal generalisation U is false. Now the probability that all the first N drawings will be white is in this case $\left(1 - \dfrac{m}{n}\right)^{N}$. Now, if $\dfrac{m}{n}$ is finite, this does tend to 0 as N tends to infinity. But suppose that n, the number of counters in the bag, tends to infinity as well as N, the number of drawings, whilst m is finite. Then this probability does not necessarily tend to 0 as N tends to infinity, for it assumes the indeterminate form 1^{∞}. Suppose we put $N = kn$ and allow n to increase without limit. Then the expression $\left(1 - \dfrac{m}{n}\right)^{N}$ becomes e^{-km}, *i.e.*, $e^{-\frac{mN}{n}}$. Now this might have any value between 0 and 1 according to whether the ratio between the two infinitely great quantities N and n was great or small.

Theorem 5.—Laplace's Rule of Succession.—As usual, this theorem divides into two. They may be called respectively (5·1) *The Non-numerical Rule,* and (5·2) *The Numerical Rule,* of Succession. I have dealt with them fairly fully in my Presidential Address to the *Aristotelian Society*, which will be found by anyone whom it may interest to look for it in Vol. XXVIII of their *Proceedings.*

(5·1) *The Non-numerical Rule.*—If N trials of Q's have been made under Bernoullian conditions and all of them have been R, then the probability that the $(N + 1)$th Q to be tried will be an R approaches the limiting value 1 as N is indefinitely increased. The formal statement in our notation is

$$(\varepsilon) :: . (\exists\nu) :: N > \nu \supset_{N} :. R(x_{N+1}) / : f_{N}(R ; Q) = 1 . \& . k : \geqslant 1 - \varepsilon.$$

The condition under which this proposition holds is that $R(x)/Q(x) \& h = 1 . /k$ shall be greater than 0.

The following example of drawing counters from a bag will make the general line of reasoning plain. The longer the series of drawings, if all of them turn out to be white, the more likely it is that the bag from which the drawings are made contains either nothing but white counters or at any rate very nearly 100 % of white counters. But on the first alternative the next drawing *must* be white, and, on the second, it is very highly probable that it will be white.

The formal argument is as follows. We divide the interval between 0 and 1 into the usual set of a very large number μ of very short adjoined sub-intervals, each of length η ; and we denote the proposition that the value of $R(x)/Q(x)$ & h lies between $r\eta$ and $(r+1)\eta$ by P_r. We denote the proposition $f_N(R ; Q) = 1$ as usual by $P_N(1)$. Then, by Lemma V,

$$R(x_{N+1})/P_N(1) \,\&\, k \,.=\, . \underset{r=0}{\overset{r=\mu-1}{S}} \, P_r/P_N(1) \,\&\, k$$
$$\times R(x_{N+1})/P_r \,\&\, P_N(1) \,\&\, k. \quad (1)$$

Now, for reasons which were stated at length in the proof of the *Statistical Principle of Greatest Probability* in Part II of this paper, the factor $P_N(1)$ is irrelevant, in conjunction with P_r, in the supposal of $R(x_{N+1})/P_r$ & $P_N(1)$ & k. Such terms as this can therefore be written in the simpler form $R(x_{N+1})/P_r$ & k.

But, by the *Inverse Principle of Great Numbers*, when N tends to infinity such terms as $P_r/P_N(1)$ & k tend to 1 for $r = \mu - 1$ and to 0 for all other values of r, provided only that $P_{\mu-1}/k$ is not zero. Therefore, provided that $P_{\mu-1}/k$ is not zero, the right-hand side of the above equation reduces to $R(x_{N+1})/P_{\mu-1}$ & k as N tends to infinity. But $P_{\mu-1}$ is the proposition that $R(x)/Q(x)$ & h lies in the very small interval between $1 - \eta$ and 1. Therefore as N tends to infinity $R(x_{N+1})/P_{\mu-1}$ & k approaches indefinitely nearly to 1. Therefore the left-hand side of the above equation approaches indefinitely nearly to 1 as N is increased indefinitely, provided only that the probability with respect to k that $R(x)/Q(x)$ & $h = 1$ is not zero. And this is what we had to prove.

(5·2) *The Numerical Rule.*—If N trials of Q's have been made under Bernoullian conditions and all of them have been R, then the probability that the $(N + 1)$th Q to be tried will be an R is $\dfrac{N+1}{N+2}$, provided that all the possible values from 0 to 1 of $R(x)/Q(x)$ & h are equally likely with respect to k.

What we have to prove, then, expressed in our abbreviated notation, is that

$$R(x_{N+1})/P_N(1) \,\&\, k = \frac{N+1}{N+2}$$

provided that P_r/k has the same value for all values of r.

The proof is as follows. If we apply Lemma VII (the *Bayes Principle*) to the factors $P_r/P_N(1)$ & k in the expression on the right-hand side of Equation (1) above, and if we remember that the term $P_N(1)$ can be suppressed in the supposal of the factors

$R(x_{N+1})/P_r$ & $P_N(1)$ & k, we find that Equation (1) can be transformed into

$$R(x_{N+1})/P_N(1) \& k = \frac{\overset{r=\mu-1}{\underset{r=0}{S}} P_r/k \times P_N(1)/P_r \& k \times R(x_{N+1})/P_r \& k}{\overset{r=\mu-1}{\underset{r=0}{S}} P_r/k \times P_N(1)/P_r \& k}. \quad (2)$$

We can evaluate all the terms in this except P_r/k. And, on the supposition that P_r/k has the same value for all values of r, the P_r/k terms in the numerator and the denominator cancel each other out. The terms to be evaluated are $R(x_{N+1})/P_r$ & k and $P_N(1)/P_r$ & k. The first is $\dfrac{r}{\mu}$; and, since the conditions of the experiment are assumed to be Bernoullian, the second is $\left(\dfrac{r}{\mu}\right)^N$. Therefore the expression on the right of Equation (2) reduces to

$$\frac{\overset{r=\mu-1}{\underset{r=0}{S}} r^{N+1}}{\mu \overset{r=\mu-1}{\underset{r=0}{S}} r^N}.$$

Now μ is enormously great, for it is the number of very short sub-intervals of length η into which we divided the interval from 0 to 1. And it is easy to prove by elementary algebra that the fraction to which the right-hand side of Equation (2) has just been reduced becomes equal to $\dfrac{N+1}{N+2}$ as μ is indefinitely increased. So this is the value of $R(x_{N+1})/P_N(1)$ & k, provided that all values of $R(x)/Q(x)$ & k are equally likely with respect to k. Q.E.D.

Theorem 6.—The rate at which the probability of a universal generalisation increases with each additional confirmatory instance diminishes as the number of confirmatory instances is increased.

This is an immediate consequence of the equation at the beginning of the proof of Theorem 4·1 and the Non-numerical Rule of Succession. For the former tells us that the ratio of U/k & $P_{N+1}(1)$ to U/k & $P_N(1)$ is *inversely proportional* to $R(x_{N+1})/P_N(1)$ & k. And the latter tells us that $R(x_{N+1})/P_N(1)$ & k increases to the limiting value 1 as N is indefinitely increased.

Theorem 7.—This is a theorem connecting the relative 'generality' of two universal propositions with their relative antecedent

probabilities. It divides into two, but I shall show that the two are logically equivalent.

We can compare two universal propositions in respect of ' generality ' if either (i) both have the same predicate, but one has a more restricted subject than the other ; or (ii) both have the same subject, but one has a more restricted predicate than the other. An example of the first case would be the two propositions ' All men are liars ' and ' All black men are liars '. We can describe the latter as ' super-determinate in respect of its subject ' to the former. An example of the second case would be the two propositions ' All men are fools ' and ' All men are fools and knaves '. We can describe the latter as ' super-determinate in respect of its predicate ' to the former.

Now it is easy to prove the following general proposition : ' If p implies q, then, whatever h may be, q/h is greater than p/h unless either (i) $p/h = 0$ or (ii) $p/q \& h = 1$ '. The proof is as follows.

If $p \supset q$ then $p \equiv p \& q$.

Therefore, by Lemma III and Postulate (v),

$$q/h = \frac{p/h}{p/q \& h}.$$

Now, by Postulate (ii) $p/q \& h$ cannot be greater than 1. Therefore, unless it is equal to 1, it must be less than 1. Therefore, unless $p/h = 0$, the expression on the right-hand side of the equation must be greater than p/h. Therefore q/h is greater than p/h unless $p/h = 0$ or $p/q \& h = 1$.   Q.E.D.

It is of some interest to consider what happens if the condition that p/h is not equal to 0 breaks down. An immediate consequence of Lemma V and Postulate (ii) is that if $p/h = 0$ then either $p/q \& h = 0$ or $q/h = 0$, whatever q may be. So, if $p/h = 0$, either $q/h = 0$ also or q/h assumes the indeterminate form $\frac{0}{0}$.

Theorems (7·1) and (7·2) are immediate consequences of the general proposition which we have just proved. They may be stated as follows.

(7·1) If p and q are any two universal propositions with the same *subject*, and the predicate of p is super-determinate to that of q, then the probability of p is *less than* that of q with respect to any datum h, unless $p/h = 0$ or $p/q \& h = 1$.

(7·2) If p and q are any two universal propositions with the same *predicate*, and the subject of p is super-determinate to that

of q, then the probability of p is *greater than* that of q with respect to any datum h, unless $q/h = 0$ or $q \mid p \, \& \, h = 1$.

The proof is obvious. In the first case (*e.g.*, where p is of the form ' All S is P ' and q is of the form ' All S is P or Q ') p entails q. In the second case (*e.g.*, where p is of the form ' All S which is P is Q ' and q is of the form ' All S is Q ') q entails p. The two theorems then follow at once from the general proposition proved above.

Both are in accordance with common-sense. The proposition with the less determinate predicate runs less risk of refutation because of the comparative *vagueness* of what it asserts. The proposition with the more determinate subject runs less risk of refutation because of the comparative *narrowness* of the field within which it asserts the predicate.

It is perhaps worth while to remark that, so far from there being any conflict between the two criteria, they logically entail each other. For suppose we start with the pair ' All S is P ' and ' All S is P or Q ', in which there is a common *subject* and where the predicate of the first is super-determinate to that of the second. Each of these propositions is logically equivalent to its contrapositive. Now their contrapositives are respectively ' All $\overline{P}$ is $\overline{S}$ ' and ' All $\overline{P} \, \& \, \overline{Q}$ is $\overline{S}$ '. These have a common *predicate*, and the subject of the second is super-determinate to that of the first.

Theorem 8. *Curve-fitting.*—I find Hr. von Wright's treatment of this subject very unsatisfactory. In the first place, I think it is vitiated by an elementary mathematical oversight, which I will explain. Secondly, even when this is avoided, as we shall see that it can be, the rest of the argument is to me (and to others far more competent than myself whom I have consulted) extremely obscure. I shall therefore have to construct an argument of my own, which is suggested by Hr. von Wright's obscure statements and leads to the same kind of conclusion as his, but is certainly not to be found in his book. Possibly it is what he has in mind.

First for the mathematical ' howler ', as it appears to me to be. The essence of the matter is as follows. Hr. von Wright supposes that we have n pairs of correlated values of two variables, x and y, given by observation, and that we are trying to find a curve which will fit them all exactly. He explicitly confines his attention to curves of the form

$$y = A_0 x^m + A_1 x^{m-1} + \ldots A_m.$$

Now it is of the essence of his argument that there might be *two* curves of this form, *viz.*, polynomials in x, fitting the *same n*

points exactly, for *both* of which m is less than n.   But it is easy to show that this supposition is logically impossible.

For suppose that all the n points were on a curve of this form, where m was less than n.   Consider any other curve of the same form but of higher order $m + p$, where $m + p$ is also less than n.   This would cut the former curve in *only* $m + p$ points, *viz.*, those which are given by the roots of the equation

$$A_0 x^{m+p} + \ldots A_{p-1} x^{m+1} + (A_p - B_0) x^m + (A_{p+1} - B_1) x^{m-1}$$
$$+ \ldots (A_{p+m} - B_m) \neq 0.$$

Hence only $m + p$ of the n points which are on the curve of the mth order could be also on the curve of the $(m + p)$th order. And, by hypothesis, $m + p$ is less than n.

This objection would not hold if Hr. von Wright were comparing *two* sets of n points, one from one experiment and the other from another, and were supposing that a polynomial of the mth order fitted the former set whilst one of the $(m + p)$th order fitted the latter.   But this is not what he says or what his argument presupposes.   What he says, and what his argument presupposes, is what I have just shown to be logically impossible.

In order to continue the discussion let us, however, suppose henceforth that we are considering *two* sets of n observations, one from one experiment and the other from another.   I propose to substitute for Hr. von Wright's very obscure argument the following reasoning, which is quite clear and, I believe, valid.

Suppose that two experiments are done, each on a different natural phenomenon.   In each of them n pairs of correlated values of two variables, x and y, are observed.   Suppose that in the first case all the observed values fall on a certain polynomial of order m ; and that in the second they all fall on a certain polynomial of order $m + p$, where both m and $m + p$ are less than n.   Denote these two propositions respectively by $O(m, n)$ and $O(m + p, n)$.   Let $L(m)$ be the proposition ' The law of the phenomena in the first experiment is the polynomial of order m which fits the n observations made in that experiment '.   Let $L(m + p)$ have a similar meaning, *mutatis mutandis*, for the second experiment.   Let h be any relevant information that we have antecedent to $O(m, n)$ and $O(m + p, n)$.   We wish to compare the two probabilities

$$L(m)/O(m, n) \,\&\, h \quad \text{and} \quad L(m + p)/O(m + p, n) \,\&\, h.$$

Consider the former of these.   If we bear in mind the fact that $O(m, n)/L(m) \,\&\, h = 1$, by Postulate (iii), since $L(m)$ implies

$O(m, n)$, we can prove at once from Lemma VII (the *Bayes Principle*) that

$$L(m)/O(m, n) \& h = \frac{L(m)/h}{L(m)/h + \overline{L}(m)/h \times O(m, n)/\overline{L}(m) \& h}.$$

Now consider the factor $O(m, n)/\overline{L}(m) \& h$ in the denominator of this. We have used up m out of our n observed values in determining the coefficients in the polynomial. So we are left with $n - m$ which might or might not fall on this curve. Now antecedently it might be that 0 or 1 or . . . $n - m$ of these would fall on the polynomial determined by the remaining m. The possibility that r of them do so covers as many possibilities as there are different ways of choosing r things out of $n - m$, *i.e.*, $^{n-m}C_r$. So the total number of such possibilities is $\overset{r=n-m}{\underset{r=0}{S}} {}^{n-m}C_r$, *i.e.*, 2^{n-m}. Of these $O(m, n)$ is a single one. So, if all of them are equally probable on the supposition that the law $L(m)$ is *false*, we have $O(m, n)/\overline{L}(m) \& h = \frac{1}{2^{n-m}}$. Therefore

$$L(m)/O(m, n) \& h = \frac{L(m)/h}{L(m)/h + [1 - L(m)/h]\frac{1}{2^{n-m}}}.$$

By precisely similar reasoning we can show that

$$L(m + p)/O(m + p, n) \& h = \frac{L(m + p)/h}{L(m + p)/h + [1 - L(m + p)/h]\frac{1}{2^{n-m-p}}}.$$

Now suppose that the antecedent probability of $L(m)$ and $L(m + p)$ is the same, *i.e.*, that $L(m)/h = L(m + p)/h = $ (say) α. Put $\frac{1 - \alpha}{\alpha} = \lambda$. Then

$$L(m)/O(m, n) \& h = \frac{1}{1 + \dfrac{\lambda}{2^{n-m}}}$$

and

$$L(m + p)/O(m + p, n) \& h = \frac{1}{1 + \dfrac{\lambda}{2^{n-m-p}}},$$

i.e., the probability that the polynomial of lower order which fits the n observations of the first experiment is the law of the phenomena examined in that experiment is greater than the probability that the polynomial of higher order which fits the

n observations of the second experiment is the law of the phenomena examined in *that* experiment. (It should be observed that this conclusion has been reached only subject to two assumptions about equi-probability.)

There is one other remark which I will make before leaving this topic. Hr. von Wright discusses the question on the hypothesis that the empirically determined points fall *exactly* on this, that, or the other suggested polynomial curve. It seems to me that this case is hardly worth considering. In real life it is a question of comparing the degree of ' goodness of fit ' of a number of alternative curves, none of which exactly fit all the observed points. Any adequate treatment of this problem would involve discussing the Method of Least Squares, about which there is an enormous literature.

This concludes what I have to say about the Formal Analysis of Inductive Probability. It is evident that we are left with two problems. One is the interpretation to be put on probability propositions. The other is the evidence, if such there be, for the truth of the conditions under which the various theorems have been deduced. It seems plain that the former question should be considered before the latter.

(2) *Interpretations of Probability Propositions.*—Hr. von Wright distinguishes the following main interpretations of the formal postulates of the calculus of probability. (i) *The Frequency Interpretation ;* (ii) *The ' Spielraum ' Interpretation ;* and (iii) *The Interpretation of Probability as an Indefinable Special Notion.* He subdivides the ' Spielraum ' Interpretation first into two forms, which he calls ' logical ' and ' empirical '. I intend, for reasons which will appear in due course, to describe them respectively as (ii, *a*) *Purely Quotitative*, and (ii, *b*) *Partly Quantitative*. Lastly, he distinguishes two sub-species of the purely quotitative form of the ' Spielraum ' interpretation. I shall call these (ii, *a*, α) the *Intrinsic*, and (ii, *a*, β) the *Extrinsic* forms of the theory.

There is one general remark to be made before we explain these various interpretations in detail. We must remember that an ' interpretation ' of a set of postulates, in the technical sense, means no more than a set of entities of any kind which, when substituted for the x's and y's and R's of the postulates, turn the latter into true propositions. They need not be in the least what anyone has in mind when he uses the words in which the axioms are stated. Thus, *e.g.*, a perfectly satisfactory ' interpretation ' of the postulates of Euclidean geometry arises if we substitute for the word ' point ', wherever it occurs, an

ordered triad of any three numbers (x, y, z); and for the phrase
'distance between two points' the square root of the sum of
the squares of the differences between the corresponding numbers
in two such triads.   But no one in his senses would suggest
that the former is what a person has in his mind in ordinary
life when he uses the word 'point' or that the latter is what he
has in mind when he uses the word 'distance'.   It is important
to face this fact and recognise it where it is obvious, as in the
case of geometry, before entering the much more obscure region
of probability where it might not be noticed.

(i) *Frequency Interpretation.*—On this interpretation the
notion of probability applies strictly and primarily only to
propositional functions and not to *propositions*.   To say that
the probability of a thing or event being an instance of R,
given that it is an instance of Q, is p means, on this inter-
pretation, that $f_N(R ; Q)$ approaches a limit as N is in-
definitely increased, and that this limiting value is p.   Some
writers, *e.g.*, von Mises, introduce a further condition, *viz.*, that
the distribution of instances of R among the instances of Q
must be in a certain sense 'random'.   Hr. von Wright has
dealt with this latter contention in his article on *Probability*
in MIND, Vol. XLIX, No. 195, and the reader may be referred
to it and to my review of von Mises in MIND, Vol. XLVI, for a
discussion of this subject.   It is not of importance in relation
to the question whether induction can be justified in terms of
probability.

It is known that the probability postulates are necessary
propositions if probability is interpreted to mean limiting
frequency.   A proof will be found in Reichenbach's *Wahrschein-
lichkeitslehre.*   There is just one remark that seems to me to be
worth making on this point.   So far as I can see, Postulates
(v) and (vi), *i.e.*, the Conjunctive and the Disjunctive Postulates,
are in a different position on the frequency interpretation.
Postulate (v) becomes a purely *algebraic* triviality, depending
simply on the identity that $\frac{a}{b} = \frac{a}{c} \times \frac{c}{b}$ where a, b, and c are any
numbers that you please.   But Postulate (vi) depends on a
certain necessary proposition about the number of terms in a
disjunctive class, *viz.*, that

$$Nc'(\alpha \vee \beta) = Nc'\alpha + Nc'\beta - Nc'(\alpha \& \beta),$$

where α and β are any *classes* that you please.   If I am right
in saying this, it seems to cast doubt on whether the frequency
interpretation expresses what we ordinarily mean by probability.

For we certainly do not regard these two postulates as being fundamentally different in character.

It is commonly held that the frequency interpretation cannot plausibly be regarded as expressing what we have in mind when we ascribe probabilities (i) to singular propositions, such as ' Mr. Jones, who has just been taken ill with influenza, will recover ', and (ii) to general laws or theories, like the Newtonian Theory of Gravitation.   In the former it would be claimed by opponents that statistical information about the proportion of recoveries in various classes of cases to which Mr. Jones's case belongs are *evidence for* ascribing such and such a probability to the proposition about Mr. Jones, but are not the whole of the meaning of such an ascription.   In the latter it would be said that no plausible interpretation in terms of frequency has ever been suggested.

Hr. von Wright, who is inclined to accept the frequency interpretation as adequate, deals with the question of the probability of universal generalisations in the following way. Consider the generalisation, ' All swans are white '.   This is a particular instance of a wider generalisation, ' All birds of the same species have the same kind of pigmentation '.   In assigning a meaning to the statement, ' The probability that all swans are white is *p* ', we have to proceed as follows.   Instead of considering *individuals of a certain species* (*e.g.*, this, that, and the other swan) and asking what proportion of them have a *certain colour* (*e.g.* white), we have to consider *species of a certain genus* (*e.g.*, the species swan, the species crow, and other species of bird) and ask what proportion of them have *sameness of pigmentation for all members* (*e.g.*, whiteness for all swans, blackness for all crows, etc.).   Suppose that the limiting proportion in the latter case is *p*.   Then this is what we mean by saying of any generalisation of the form ' All members of such and such a species of birds (*e.g.*, swans) have such and such a kind of pigmentation (*e.g.*, uniform whiteness) ' that its probability is *p*.

Hr. von Wright realises that the conditions here laid down can never, strictly speaking, be fulfilled.   As regards an individual, *e.g.*, a swan, one can decide with certainty by looking at it whether it does or does not have a certain colour all over, *e.g.*, whiteness. But we cannot examine all members, past, present, and future, of *any* species, and therefore cannot know that it has a certain property common to all its members.   To this he answers that we may know, with regard to the N species of a genus, instances of which have been examined, that a proportion 1 − *p lacked* internal uniformity in respect of a certain generic quality (*e.g.*,

colour). And we may know that the remaining pN of them had such uniformity *for all their members examined up to date*. On this kind of basis, which is all that from the nature of the case we could ever have, the most reasonable estimate of the probability of any such generalisation as 'All swans are white' is p. And the *meaning* of such a statement as 'The probability that all swans are white is p' remains unaltered by these further refinements.

I have no doubt whatever that this kind of reference to wider classes is often a most important part of the *evidence for* a universal generalisation. One would be inclined, *e.g.*, to feel less confidence in such a generalisation as 'All swans are white' than in such a one as 'All samples of phosphorus melt at 40° C.' on the ground that experience has shown that pigmentation is a more variable quality among birds of the same species than is melting-point among samples of the same chemical element. But it is not clear to me how the notion of *limiting* frequency, which is Hr. von Wright's proposed interpretation of probability, is to be applied to such a collection as the various species of the genus bird and to such a characteristic as 'sameness of pigmentation within a species'. This notion is clear enough when we have something analogous to the potentially unlimited sequence of drawings and replacements of a counter from a bag. But surely the analogy has worn very thin here.

(ii) *The 'Spielraum' Interpretation.*—Let us call any proposition which is explicitly constructed from one or more other propositions by the single or repeated application of some or all of the processes of negation, conjunction, or disjunction 'explicitly molecular'. A proposition which is not explicitly molecular will be called 'ostensibly atomic'. The ostensibly atomic propositions out of which an explicitly molecular proposition is constructed will be called its 'elements'. Each of the elements of an •explicitly molecular proposition may be either true or false. So, if there are n of them, there will be 2^n alternative possible combinations of truth-values for them. Each such alternative will be called an 'elementary truth-value combination'. Any explicitly molecular proposition will be true for certain of the truth-value combinations of its elements and will be false for the rest of them. All forms of 'Spielraum' interpretation depend on the facts just described. In the *purely quotitative* variety of the theory the alternative possibilities are merely counted. They are not regarded as each having a certain kind of magnitude—degree of 'possibility' or of 'probability' —in respect of which they can be judged to be equal or unequal.

In the *partly quantitative* variety the alternatives are compared in respect of such a magnitude before being counted.

(ii, *a*, α) *Intrinsic Form of Purely Quotitative Variety.*—On this form of the theory the ' probability ' of any proposition is defined as the ratio of the number of the truth-value combinations of its elements which *make it true* to the *total* number of such combinations. This makes the probability of every ostensibly atomic proposition to be $\frac{1}{2}$. For there are two possibilities, true or false ; and the possibility of its being true is one of them. Next, consider the conjunctive proposition $p \& q$. With two elements there are four elementary truth-value combinations. One only of these, *viz.*, *p* true and *q* true, makes $p \& q$ true. Therefore the probability of $p \& q$ is $\frac{1}{4}$. Lastly, consider the disjunctive proposition $p \vee q$. Of the four elementary truth-value combinations all but one, *viz.*, *p* false and *q* false, make $p \vee q$ true. Therefore the probability of $p \vee q$ is $\frac{3}{4}$. It is evident from this that the Disjunctive Postulate holds with this interpretation of probability. For, as we have just seen, the probability of $p \vee q$ is $\frac{3}{4}$; that of the two atomic propositions *p* and *q* is $\frac{1}{2}$ in each case ; and that of the conjunctive proposition $p \& q$ is $\frac{1}{4}$. And, by simple arithmetic, $\frac{3}{4} = \frac{1}{2} + \frac{1}{2} - \frac{1}{4}$. In a similar way it can be shown that all the other postulates hold.

I call this form of the quotitative variety of the theory ' intrinsic ', because it does not make the probability of a proposition to be or to involve a relation to any other proposition.

(ii, *a*, β) *Extrinsic Form of Purely Quotitative Variety.*—Let *P* be any proposition and *H* be any other proposition. Either of them may be either ostensibly atomic or explicitly molecular. On the present theory the probability of *P* given *H* is defined as the ratio of the number of truth-value combinations of the elements of the conjunctive proposition $P \& H$ which make *that conjunction true* to the number of them which make *H true*. This definition will be made clearer by some examples. In the first place, the probability of any ostensibly atomic proposition *p* with respect to any other ostensibly atomic proposition *h* will be $\frac{1}{2}$. For there are four elementary truth-value combinations with the two propositions *p* and *h* ; and one of them makes $p \& h$ true, whilst two of them make *h* true. Again, consider $p \vee q/h$. With three elementary propositions there are eight elementary truth-value combinations. Three of these make the conjunction $p . \vee . q : \& h$ true, whilst four of them make *h* true. So the probability of $p \vee q/h$, on the present theory, is $\frac{3}{4}$, provided that *p*, *q*, and *h* are all ostensibly atomic propositions. It is easy to show that all the Postulates are satisfied with this interpretation of P/H.

I call this form of the quotitative variety of the theory ' extrinsic ', because it makes the probability of a proposition to be something which is essentially relative to another proposition.

(ii, *b*) *Partly Quantitative Variety*.—The ' Spielraum ' theory has very little plausibility as an account of what we commonly have in mind when we talk of ' probability ' so long as it remains purely quotitative.   To make it plausible we have to add the condition ' provided that all the alternative possibilities considered and counted are *equally* probable '.   Hr. von Wright says that at this stage the theory does not differ from the classical Laplacean definition.   If it does not, I should say that it is an extremely clumsy way of putting that definition.   The normal and straightforward way of putting it would be as follows.

According to it the statement ' $p/h = \dfrac{m}{n}$ ' means that h is a disjunction of n mutually exclusive and equi-probable alternatives whilst p is logically equivalent to a disjunction of m of these. This would, of course, be circular if it were offered as a definition of *probability*.   But it is not circular if it is content to take the notion of probability as undefined and merely claims to define the statement that the *degree* of probability of a proposition is *so-and-so*.   With this restriction it seems to me to answer exactly to our practice in working out problems in the Theory of Probability.   But the proviso that the alternatives shall be equi-probable is absolutely essential, and this notion remains undefined.   Moreover, the definition does not suggest any criterion by which we are to judge in any concrete application whether two alternatives are or are not equiprobable.   At this point the theory has either to rely on individual intuition, or to appeal to some general principle such as that of Symmetry or that of Insufficient Reason, or to base its judgments of equi-probability on the relative frequency with which the various alternative possibilities have been realised.   The first two alternatives are unsatisfactory, whilst the third brings us back to the Frequency Theory and to the problem of Statistical Generalisation and its justification.

(iii) *Probability as an Indefinable Notion*.—Hr. von Wright mentions this alternative and points out that it cannot provide an ' interpretation ' of the Postulates in the technical sense, as the Frequency and the ' Spielraum ' theories certainly do. That is to say, the Postulates do not become necessary propositions of arithmetic or of the logic of classes, as they do if probability is interpreted either in accordance with the Frequency

Theory or the several varieties of the 'Spielraum' Theory. He does not pursue this alternative further; and, in particular, he does not consider the contention that the probability relation is analogous to but 'weaker than', the relation between the premiss and the conclusion of a valid deductive inference.

(3) *Restatement of the Laws of Great Numbers in Terms of the Frequency Theory.*—Before going further I think it will be very well worth while to do something which Hr. von Wright does not attempt, *viz.*, to restate the Direct and the Inverse Laws of Great Numbers in terms of the Frequency Theory of Probability. For the sake of simplicity and concreteness I shall take the particular examples of drawing counters from bags rather than the general propositions of which these are instances.

(3·1) *Direct Law of Great Numbers.*—The essential point to notice is that we have to consider two different sequences, *viz.*, (i) a sequence of *drawings*, and (ii) a sequence of *sets of drawings*. In the first we are concerned with the limiting frequency of *white drawings*, and in the second with the limiting frequency of *sets containing a certain proportion of white drawings*. This being understood, the Direct Law may be stated as follows:

Suppose that the proportion of *white drawings* in a sequence of N *drawings* made under Bernoullian conditions from a certain bag of counters approaches to the limit p as N is increased indefinitely. Then, however small δ may be, the proportion of *sets of n drawings containing between $n(p - \delta)$ and $n(p + \delta)$ white drawings* in a sequence of N' *sets of n drawings* from that bag will approach to the limit 1 as N' is increased indefinitely.

(3·2) *Inverse Law of Great Numbers.*—Here there are two additional points to bear in mind. (i) This law is concerned with the probability of a probability. On the Frequency Theory this must be the limiting frequency with which a certain limiting frequency occurs. (ii) The condition under which the Law holds is that it shall not be infinitely improbable that the value of a certain probability shall lie in the immediate neighbourhood of a certain fraction. This condition will have to be expressed in terms of limiting frequencies. With these preliminaries the Law may be stated as follows.

Suppose that from each of N' bags of unknown constitution a sequence of N drawings has been made under Bernoullian conditions, and that in each of these sequences the proportion of white drawings has been p. Suppose that, in such circumstances as those under which the experiment was done, bags containing a proportion p of white counters are not infinitely

rare. Then, however small δ and ϵ may be, if N' and N be sufficiently great the proportion of these N' bags in which the proportion of white counters lay between $p - \delta$ and $p + \delta$ cannot have differed from 1 by more than ϵ.

I am afraid that this is a very complicated statement; but I think that it is clear, and I do not see how to express the Inverse Law of Great Numbers in terms of the Frequency Theory in any simpler way.

(4) *The Alleged ' Bridge ' between Probability and Frequency.*— It has often been suggested that, even if probability cannot be *defined* in terms of frequency, yet the Direct Law of Great Numbers provides a ' bridge ' from the former to the latter. If this means that the Law enables one to start from a premiss which asserts probability and to infer a conclusion which categorically asserts that such and such a frequency will be or has been realised, it is a complete mistake. A moment's inspection of the accurate formulation of the Direct Law will show this. The premiss is that the probability of any *single* event of a certain generic kind turning out in a certain specific way is so-and-so. The conclusion is that the probability of such and such a *proportion of events* of that kind turning out in that way approaches to such and such a limit as the length of the sequence is indefinitely prolonged. Thus the inference is from probability to probability, not from probability to frequency. This is in no way altered by the fact that the frequency which is the subject of the probability-statement in the conclusion is the same fraction as the probability which is asserted of the individual event in the premiss. Nor is it altered by the fact that the probability which is mentioned in the conclusion is there asserted to differ by as little as we please from 1 provided that the sequence is sufficiently prolonged.

Again, the Inverse Law of Great Numbers does not provide a ' bridge ' from frequency to *first-order* probability, but only to the probability of another probability having a certain numerical value. The premiss here is that, in a sequence of events of a certain generic kind, a certain proportion have turned out in a certain specific way. The conclusion is that it is to such and such a degree probable that the probability of any single event of this kind turning out in this way was so-and-so. The argument, then, is from a frequency to a *second-order* probability. This is in no way altered by the fact that the first-order probability which forms the subject of the conclusion is there shown to be more likely to have the same numerical value as the frequency which is asserted in the premiss than to have any other value.

14

Nor is it altered by the fact that the probability of its having that numerical value is there asserted to differ by as little as we please from 1 provided that the sequence mentioned in the premiss was long enough.

Hr. von Wright mentions certain linguistic confusions which he thinks may have caused intelligent persons to make the mistakes which we have been pointing out. Whatever the cause may be, the best cure is simply to state the two Laws of Great Numbers with meticulous accuracy, and then to translate them with equal care in terms of whatever interpretation of 'probability' one may like to adopt. I have tried to do this in the previous Section for the Frequency interpretation.

(5) *Can Inductive Generalisation be justified by the Principles of Probability ?*—If this can be done at all, it must be done by means of the Theorems which we have proved above; and all the rest of these depend upon the Direct and the Inverse Laws of Great Numbers. Now we have just seen that, whatever interpretation we may put on 'probability', these laws will at best enable us to infer only that under certain conditions certain statistical or universal generalisations will have a probability which approaches in the limit to 1. So we can confine ourselves to the contention that, in favourable circumstances, inductive generalisation can be justified with high probability. But this means nothing in particular until some interpretation has been put upon the notion of probability. For the present purpose we may divide the possible interpretations into (i) the Frequency Interpretation, and (ii) Non-frequency Interpretations.

On the Frequency Interpretation it is certain that the more probable it is that an event of a given kind will turn out in a certain way the greater is the relative frequency with which events of that kind will turn out in that way if the sequence of such events be sufficiently prolonged. This is certain simply because, on this interpretation of probability and on this alone, it is analytic. On the other hand, the Direct Law of Great Numbers starts with a premiss about the probability of events of a certain kind turning out in a certain way. Now, on the Frequency Interpretation, this is itself a statement about the relative frequency with which events of that kind will turn out in that way as the sequence of such events is indefinitely prolonged. Either this is merely assumed as a hypothesis, or it is taken as a categorical premiss. On the first alternative anything that is inferred from it is shown only to be a consequence of the hypothesis ; it cannot be asserted by itself as a categorical conclusion. On the second alternative we are at once faced with

the question : What is your evidence for the proposition about limiting frequency which is asserted in your premiss ?   Plainly no proposition of this kind can be a mere report of what has been perceived, as, *e.g.*, the proposition, ' This swan is white ' or ' All the swans in this pond are white ', might be.   It is in fact evident that the premiss is itself a statistical generalisation from the observed frequency of certain kinds of events in certain limited sequences.   To put it briefly.   On the frequency interpretation of probability the Theorems will enable you to pass from premisses about limiting frequencies in certain sequences to conclusions about limiting frequencies in certain other sequences related to the former in certain specified ways.   Such inferences are of great interest and importance.   But, since the premisses are themselves either assumed as hypotheses or established by inductive generalisation, these Theorems cannot supply a justification for inductive generalisation in general.

Suppose, then, that we put some *non-frequency* interpretation on probability.   Then the mere fact that the probability of an event of a certain kind turning out in a certain way is p is no *guarantee* that, even in the long run, events of that kind will turn out in that way with the relative frequency p.   No doubt, if the run is long enough, it is overwhelmingly probable (in whatever sense ' probable ' is being used) that the proportion of such events which turn out in this way will be approximately p.   But (except on the frequency interpretation, which we are now excluding) this does not entail that in a very long series of very long equal runs of such events an overwhelming proportion of the runs *would* contain a proportion p of events which had turned out in this way.   This too can, no doubt, be shown to be overwhelmingly probable (in whatever sense ' probable ' is being used) ; and so on without end.   But at no stage shall we be able to pass from a certain frequency being overwhelmingly probable, in the non-frequency sense, to its being overwhelmingly frequent.

The upshot of the matter is this.   The fact that one alternative is more likely to be fulfilled than another is a good reason for acting on the assumption that the former will be realised only if on the whole and in the long run the more probable alternative is the one that is more often realised.   This condition is guaranteed analytically on the frequency interpretation, and is not guaranteed at all on any other interpretation, of probability. On the other hand, if we interpret probability in terms of limiting frequency, we must abandon all hope of justifying inductive generalisation by means of the Laws of Great Numbers.   For, on

that interpretation, these Laws merely enable you to pass from premisses about limiting frequencies in certain sequences to conclusions about limiting frequencies in certain other sequences connected in certain specific ways with the former. The problem : What justification is there for asserting that the limiting frequency for any particular kind of series is so-and-so ? falls outside these Theorems, just as the problem of guaranteeing the premisses of a syllogism falls outside the Theory of the Syllogism.

(6) *Induction as a Self-Correcting Process.*—The last topic with which Hr. von Wright deals is Reichenbach's contention that induction is a self-correcting process. I am not at all sure that I understand either Reichenbach's statements in his *Wahrscheinlichkeitslehre* or Hr. von Wright's synopsis of them. I propose therefore to try by means of an example to state what I imagine to be the point. If I am wrong, wiser heads will be able to correct me.

Suppose that we are given a coin, not known to be fair, and that we want to make an estimate of the probability of throwing a head with it.

(i) We throw it n times, and we find that we get m heads. At this stage we estimate the probability of throwing a head with this coin as $\dfrac{m}{n}$.

(ii) We now make a sequence of n' *sets*, each of n throws of the coin. Let the number of these sets which contain $0, 1, \dots n$ heads respectively be $n'_0, n'_1, \dots n'_n$. Then we should have

$$\underset{r=0}{\overset{r=n}{\text{S}}} n'_r = n'.$$

Now suppose that the probability of throwing a head were p. Then we can calculate what would be the most probable number of n-fold sets of throws containing exactly r heads in a sequence of n' such sets on this hypothesis. Call this v'_r. The actual value of v_r' would be ${}^nC_r p^r (1 - p)^{n-r} n'$.

I now introduce something which is entirely conjectural, for I find no explicit mention of it in either Reichenbach or Hr. von Wright. This is the well-known function χ^2 as a measure of goodness of fit.

Consider the expression

$$\chi^2 = \underset{r=0}{\overset{r=n}{\text{S}}} \frac{(n'_r - v'_r)^2}{v'_r}.$$

This measures the goodness of fit of the hypothesis that the probability of throwing a head is p to the actual distribution of heads among the sets in the sequence. The fit is closest when this expression is as small as possible. Since ν_r' is a function of p, χ^2 will also be a function of p. We want to find that value of p which will make χ^2 a minimum. Call this p_0. If p_0 does not differ from $\dfrac{m}{n}$ we have no reason to revise our original estimate of the probability of throwing a head with this coin. But, if p_0 does differ appreciably from $\dfrac{m}{n}$, it is reasonable to substitute it for $\dfrac{m}{n}$ as our estimate of this probability. For this is the hypothesis which best fits the more detailed facts now at our disposal, *viz.*, the actual *distribution* of the various possible numbers of heads in a sequence of n' sets of n throws.

(iii) We now pass to the next stage. We make a sequence of n'' sequences each consisting of n' sets of n throws. Let the number of these sequences which consist of r_0 sets containing 0 heads, r_1 sets containing 1 head, . . . and r_n sets containing n heads be denoted by $n''_{r_0 r_1 \ldots r_n}$. The r's are of course subject to the condition that

$$\mathop{S}_{k=0}^{k=n} r_k = n'.$$

Now suppose that the probability of throwing a head were p. Then we could calculate what would be the most probable number of sequences of n' sets of n throws consisting of r_0 sets with 0 heads, r_1 sets with 1 head, . . . and r_n sets with n heads. Call this $\nu''_{r_0 r_1 \ldots r_n}$.

As before we calculate χ^2 for all the possible values of the r's, and we proceed to find that value of p which makes the new χ^2 a minimum. Call this p_0'. If it does not differ from p_0, the estimate reached at the previous stage, there is no reason to revise the latter. If it does differ from p_0, it is reasonable to substitute p_0' for p_0 as our estimate of the probability of throwing a head with this coin.

(iv) The principles of the procedure are now plain, and it could be pursued to as many further stages as we like.

Supposing, for the sake of argument, that what Reichenbach had in mind is something like what I have been trying to describe, the advantages of the procedure would seem to be the following. It is true that the three stages which I have been describing might be taken to consist simply of a run of n throws, a run of

n' . n throws, and a run of n'' . n' . n throws, and that we might simply have taken the ratio of the number of heads to the number of throws in each case as successive estimates of the probability of throwing a head with this coin.   If we had done so, the three estimates would have been in ascending order of reliability because they are based on increasingly long sequences of throws. But by merely doing this we throw away much detailed information which is relevant to question at issue.   We take no account of any information that may be available about the distribution of the various possible proportions of heads among successive equal sets of throws, or about the still more complex facts of distribution among successive equal sequences of successive equal sets of throws.   It is intelligible that, by taking into account such information in some such way as I have suggested, we might reach in a shorter total sequence of throws as accurate an estimate as we could reach in a much longer sequence by cruder methods which ignore these details.

All this may be completely beside the mark ; but even if it be, it is of some interest in itself, and so I give it for what it may be worth.

CONCLUSION.—As I hope that Hr. von Wright's book may have a wide circulation in the new New Jerusalem, when the world has been made still safer for democracy, I shall end with a list of the misprints or traces of imperfect English which I have noticed in it.   The chief of these are as follows :—

P. 48, last line but one, for *phosporus* read *phosphorus*.
P. 80, line 3 of par. 5, for *it* read *if*.
P. 149, line 5 of par. 5, for *others* read *other*.
P. 233, last line of Note 19, for *Braitwaite* read *Braithwaite*.

These are the main misprints.   The English needs to be amended in the following passages :—

P. 100, l. 4, for *depending upon what* read *according to what*.
P. 105, l. 6 of par. 3, for *inefficiencies* read *defects*.
P. 106, last line, for *inclusive* read *including*.
P. 108, l. 3 of par. 4, for *arithmetics and the analysis* read *arithmetic and analysis*.
P. 137, l. 1 and l. 25, for *indirectly* read *inversely*.
P. 152, penultimate line, for *in average* read *on the average*.
P. 174, l. 5, for *constance* read *constancy*.
P. 233, Sect. 6, Note 4, for *supersede* read *exceed*.

II.—POSITIVISM.

By W. T. STACE.

1

THERE are doubtless many philosophies which, though differing
from one another in various ways, might all be properly called,
in one sense of the word or another, positivistic. Let me
therefore try to say what, in this paper, I shall mean by
positivism. The characteristic thesis of positivism, as the term
will be here understood, seems to run something like this : " A
set of words purporting to express a factual proposition P is
significant only if it is possible to deduce or infer from it, in
combination if necessary with other premises, some proposition
or propositions (at least one) Q_1, Q_2, Q_3 . . . etc., the truth
or falsity of which it would be logically possible to verify by
direct observation. If no such directly verifiable deductions
from P are possible, then the set of words purporting to express
P is non-significant, and P is not really a proposition at all,
but a pseudo-proposition." [1] I shall call this the *Positivist
Principle*, and I shall refer to any philosophy which maintains
it as positivism.

I will make two comments on the wording of the principle
as thus formulated. First, I use the words " significant " and
" non-significant ", following Russell. I wish to distinguish
between the *meaning* of a word and the *significance* of a sentence.
In common language we speak both of the meaning of a word
and of the meaning of a sentence. But the meaning of " mean-
ing " in the two cases is quite different. For the meaning of
a sentence is something to which the predicates true or false
apply. The meaning of a sentence must be either true or false.
But the predicates true and false do not apply to the meanings
of words. A single word, such as " red ", is neither true nor
false. To mark this distinction I shall call what is in common
language called the meaning of a sentence its significance. I

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[1] It might be said that this formula excludes certain admittedly signifi-
cant sentences about immediate sense-experiences from which no pre-
dictive consequences follow. Allowance could easily be made for such
sentences by adding suitable additional clauses in the formula. But I
prefer to avoid an over-complicated statement, since the point—however
important in other contexts—is not important for the argument of this
paper.

shall confine the word meaning to meaning in the sense in which single words have meaning. Sentences have significance, words have meaning. This is, in my opinion, a distinction of crucial importance. An essential part of the argument of this paper will turn upon it.

This distinction of two kinds of meaning has nothing to do with the common distinctions between semantical or referential meaning and expressive or emotive meaning, or between these and imagistic, literary, associative, or other kinds of meaning. So far as I understand the matter, the distinction between meaning and significance is a distinction *within* the genus of semantical meanings. I shall be talking only about the semantical meanings of words and the semantical significances of sentences. Nothing in this paper will be about expressive or other kinds of non-semantical meaning.

My second comment on wording is as follows. In my formulation of the positivist principle in the first paragraph I use the phrase " deduce or infer ". This is because I think that in speaking of the situation in which certain verifiable consequences can be drawn from the proposition P (I will symbolize this situation or relation hereafter by writing $P \rightarrow Q$), positivists have in mind *both* deductive arguments and causal inferences. For instance, the sentence, " water boils at 100° C.", would be said to be significant because from it can be derived deductively the sentence, " *This* pot of water will boil at 100° C." and " *That* pot of water will boil at 100° C.", and both of these propositions can be directly verified by boiling the pots of water and using the thermometer. On the other hand, " I was ill on January 1st " is significant because by appealing to certain causal sequences, I can infer from it that there will be an entry in my diary to the effect that I was ill on that date, and I can now verify this entry.

No doubt it is not *necessary* to make this distinction, since it is really allowed for by the provision that the deductions from P may be made with the help of other premises. For by including the causal law among the premises along with P, the argument may be represented as deductive. Thus I can argue, " Whenever I am ill, I enter the fact in my diary. I was ill on January 1st. Therefore there must be an entry of this fact in my diary." Nevertheless it seems desirable, in view of the subsequent argument in this paper, to point out that the procedure really covers both what are ordinarily called deductive arguments and what are ordinarily called causal inferences.

I am not sure whether I have correctly and happily expressed

the positivist principle in my first paragraph. I will therefore add that I think I understand by my formulation the very same thing which Mr. A. J. Ayer understands when he writes : " Let us call a proposition which records an actual or possible observation an experiential proposition. Then we may say that it is the mark of a genuine factual proposition . . . that some experiential proposition can be deduced from it in conjunction with certain other premises without being deducible from those other premises alone." [1] I am not conscious of any difference of meaning between Mr. Ayer's formulation and mine. So that if there is any difference, it must be either because I do not clearly understand Mr. Ayer's meaning, or because I do not clearly understand my own meaning, or both. I also think that my formulation expresses the same thought which has often previously been expressed by Carnap [2] and other writers who would ordinarily be called positivists.

2.

The recent history of the positivist principle is relevant to my purposes in this paper. A decade or more ago some writers (for example Schlick) said that the meaning (significance) of a statement is the method of its verification (the so-called principle of verifiability). By verification they apparently meant *direct* and *complete* verification. Direct verification is, I think, the same thing as observation. For instance, I directly verify the statement that the cat's eyes are blue by observing the blue eyes of the cat. It was quickly pointed out that, if direct verification were required, statements about the past would be non-significant. For it is logically impossible to observe the past now.[3] And if complete verification were required, universal propositions would be non-significant, since it is logically impossible to observe an infinite number of facts. It was even held that singular statements about material objects would be non-significant, since their complete verification would also involve an infinite number of observations. To avoid these unfortunate consequences, exponents of the principle of verifiability began to admit that *indirect* and *partial* verification would

[1] *Language, Truth and Logic*, p. 26.
[2] See, for example, Carnap, *Philosophy and Logical Syntax* (Psyche Miniature Series), pp. 9-15.
[3] Attempts were made to deny that this is *logically* impossible. I think these attempts were failures. But I do not wish to discuss this point now.

shall confine the word meaning to meaning in the sense in which single words have meaning. Sentences have significance, words have meaning. This is, in my opinion, a distinction of crucial importance. An essential part of the argument of this paper will turn upon it.

This distinction of two kinds of meaning has nothing to do with the common distinctions between semantical or referential meaning and expressive or emotive meaning, or between these and imagistic, literary, associative, or other kinds of meaning. So far as I understand the matter, the distinction between meaning and significance is a distinction *within* the genus of semantical meanings. I shall be talking only about the semantical meanings of words and the semantical significances of sentences. Nothing in this paper will be about expressive or other kinds of non-semantical meaning.

My second comment on wording is as follows. In my formulation of the positivist principle in the first paragraph I use the phrase " deduce or infer ". This is because I think that in speaking of the situation in which certain verifiable consequences can be drawn from the proposition P (I will symbolize this situation or relation hereafter by writing $P \rightarrow Q$), positivists have in mind *both* deductive arguments and causal inferences. For instance, the sentence, " water boils at 100° C.", would be said to be significant because from it can be derived deductively the sentence, " *This* pot of water will boil at 100° C." and " *That* pot of water will boil at 100° C.", and both of these propositions can be directly verified by boiling the pots of water and using the thermometer. On the other hand, " I was ill on January 1st " is significant because by appealing to certain causal sequences, I can infer from it that there will be an entry in my diary to the effect that I was ill on that date, and I can now verify this entry.

No doubt it is not *necessary* to make this distinction, since it is really allowed for by the provision that the deductions from P may be made with the help of other premises. For by including the causal law among the premises along with P, the argument may be represented as deductive. Thus I can argue, " Whenever I am ill, I enter the fact in my diary. I was ill on January 1st. Therefore there must be an entry of this fact in my diary." Nevertheless it seems desirable, in view of the subsequent argument in this paper, to point out that the procedure really covers both what are ordinarily called deductive arguments and what are ordinarily called causal inferences.

I am not sure whether I have correctly and happily expressed

the positivist principle in my first paragraph. I will therefore add that I think I understand by my formulation the very same thing which Mr. A. J. Ayer understands when he writes : " Let us call a proposition which records an actual or possible observation an experiential proposition. Then we may say that it is the mark of a genuine factual proposition . . . that some experiential proposition can be deduced from it in conjunction with certain other premises without being deducible from those other premises alone." [1] I am not conscious of any difference of meaning between Mr. Ayer's formulation and mine. So that if there is any difference, it must be either because I do not clearly understand Mr. Ayer's meaning, or because I do not clearly understand my own meaning, or both. I also think that my formulation expresses the same thought which has often previously been expressed by Carnap [2] and other writers who would ordinarily be called positivists.

2.

The recent history of the positivist principle is relevant to my purposes in this paper. A decade or more ago some writers (for example Schlick) said that the meaning (significance) of a statement is the method of its verification (the so-called principle of verifiability). By verification they apparently meant *direct* and *complete* verification. Direct verification is, I think, the same thing as observation. For instance, I directly verify the statement that the cat's eyes are blue by observing the blue eyes of the cat. It was quickly pointed out that, if direct verification were required, statements about the past would be non-significant. For it is logically impossible to observe the past now. [3] And if complete verification were required, universal propositions would be non-significant, since it is logically impossible to observe an infinite number of facts. It was even held that singular statements about material objects would be non-significant, since their complete verification would also involve an infinite number of observations. To avoid these unfortunate consequences, exponents of the principle of verifiability began to admit that *indirect* and *partial* verification would

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[1] *Language, Truth and Logic*, p. 26.

[2] See, for example, Carnap, *Philosophy and Logical Syntax* (Psyche Miniature Series), pp. 9-15.

[3] Attempts were made to deny that this is *logically* impossible. I think these attempts were failures. But I do not wish to discuss this point now.

be sufficient. I indirectly verify a past occurrence by now directly verifying or observing some of its effects. Thus I indirectly verify the fact that I was ill on January 1st by now observing the entry in my diary. And I indirectly verify the fact that it rained a few minutes ago by observing the present wetness of the ground. Also I partially and sufficiently verify the universal statement that water boils at 100° C. by directly observing particular cases of water boiling at that temperature. This, I believe, is what Carnap called " testing " the proposition.

It was in this way that the present positivist principle was reached—by toning down Schlick's original principle of verifiability so as to allow indirect and partial verification. What is now required in order to make a statement about the past significant is, not that the facts asserted in the statement should be themselves now observable, but that some of their effects should be observable (indirect verification). And what is required to make a universal proposition significant is not that *all* the facts which it asserts (an infinite number) should be observable, but that some of them should be observable (partial verification). These are the requirements which are embodied in the positivist principle as formulated in the first paragraph of this article.

3.

I shall contend that the positivist principle, which has frequently been explicitly advocated by positivist writers, really rests upon another principle which has, so far as I know, never been expressly asserted by positivists themselves, or by their critics, or by any other philosophers. This principle, which (as I believe) is always implied by positivists but never stated, will be called by me the *Principle of Observable Kinds*. I do not know whether I can succeed in stating it unambiguously or adequately. But my attempt at a formulation of it is as follows :
The Principle of Observable Kinds.

A sentence, in order to be significant, must assert or deny facts which are of a kind or class such that it is logically possible directly to observe some facts which are instances of that class or kind. And if a sentence purports to assert or deny facts which are of a class or kind such that it would be logically impossible directly to observe any instance of that class or kind, then the sentence is non-significant.

Roughly, the point of the principle may be made clear by means of an example. Consider the sentence " Napoleon crossed the Alps ". It is logically impossible now directly to observe

the particular fact asserted in the sentence, namely Napoleon-crossing-the-Alps, because that fact no longer exists. But this fact, Napoleon-crossing-the-Alps, is a member of the general class of facts " men crossing mountains ". And although it is not possible to observe the particular member of the class mentioned in the sentence, it is logically possible to observe other members of the class. That is, it is possible to observe men crossing mountains. Thus the sentence states a fact which, though not itself observable, is yet of an observable *kind*. It is, therefore, according to the principle of observable kinds, a significant sentence. But if the facts stated in the sentence were not only themselves unobservable but also belonged to a kind of facts (or alleged facts) such that it would be logically impossible ever to observe any instance of the kind, then the sentence would be non-significant. A sentence is significant if what it asserts or denies is the *sort* of thing which it is logically possible to observe, even if the particular instance in the sentence is such that it would not be logically possible to observe it. It is non-significant if this is not the case.

Consider again the sentence "water boils at 100° C.". It is logically impossible to observe what is asserted here, because what is asserted comprises an infinite number of facts. But it is possible to observe instances of the class " water-boiling-at-100° C." to which all the infinite number of possible instances asserted in the sentence belong. Water boiling at 100° C. is the *sort* of thing which can be observed. Therefore the sentence is significant.

4.

I shall later try to show that the positivist *must* accept and maintain the principle of observable kinds, because the positivist principle *implies* it. But for the moment I wish to point out that the principle of observable kinds appears on the face of it to be *not* anything which the positivist holds or maintains. For it seems to say something quite different from what the positivist principle says. The chief differences appear to be (1) that the principle of observable kinds introduces the notion of *classes* of facts, whereas the positivist principle says nothing at all about classes ; and (2) that the positivist principle makes use of the concept of indirect verification, whereas this is entirely left out of the principle of observable kinds, which contains only the concept of direct verification or observation. These two points are very closely related to one another. I will now explain them.

In a sense, the principle of observable kinds forces the posi-
tivist back upon the concept of direct verification, which he has
been attempting to avoid.   But it does not force him back
upon the old position asserted in Schlick's original principle of
verifiability already quoted.   It avoids this by introducing the
notion of classes.   Suppose the proposition to be examined for
its significance is P.   Then according to the original principle
of verifiability the facts asserted or denied in P must *themselves*
be capable of being observed.   What the new positivist principle
says is that the facts asserted or denied in Q (the proposition or
propositions deduced from P) must themselves be capable of
being observed.   *It does not say anything at all about the observ-
ability or non-observability of the facts asserted or denied in P.
It entirely ignores that question.*   Thus there is a lacuna here in
the positivist position.   Something ought to have been said
about whether the facts stated in P must be observable.   But
nothing is said.   The principle of observable kinds fills up the
lacuna.   It tells us what the positivist ought to say regarding
this question.   The positivist is anxious to avoid saying that the
facts asserted or denied in P must themselves be directly observ-
able, because this will make propositions about the past non-
significant.   On the other hand, would he be content if it were
suggested that, in the situation P → Q, it might be the case
that, although the facts asserted in Q are observable facts, yet
the facts stated in P might be such that it would be logically
impossible for anyone ever to observe any facts of that kind at
all ?   Would he admit that from a proposition P, asserting a
set of facts of a kind which it would be logically impossible to
observe, one could yet deduce Q, asserting facts which can be
observed ?   I do not think he would.   What then ought he to
say about the facts asserted in P ?   The principle of observable
kinds gives the answer.   He ought to say, not that the *particular*
facts asserted in P must be observable (this is what he wished
quite rightly to avoid), nor yet that they may be of a wholly
unobservable kind ; but rather that the particular facts asserted
in P, although they cannot themselves be observed, must yet
be of a *kind* of facts of which *other* instances can be observed.
And this is what the principle of observable kinds does say.

5.

I am not at all sure in what exact relation the principle of
observable kinds stands to the positivist principle.   I feel sure
that there is a relation of implication ; that is, the positivist

principle logically implies the principle of observable kinds ;
so that the positivist *must* admit the latter. Also I am fairly
sure that the principle of observable kinds is not only an *addi-
tional* principle of positivism, over and above the positivist
principle, but that it in some way contains the *whole* of positivism,
so that it could be *substituted* for the positivist principle, not only
without loss, but with gain. For it has over the positivist
principle a great advantage. It is a criterion of significance
which can be applied direct to the proposition which is being
tested for significance ; whereas the positivist principle can only
be applied indirectly by way of the proposition's logical con-
sequences. Hence as the principle of observable kinds is, in
this respect, an improvement on the positivist principle, I hereby
make a present of it to the positivists of the world. But I also
suggested above that the positivist principle " rests upon "
the principle of observable kinds. I am not at all sure what
this means. I feel inclined to say that the positivist principle
" presupposes " the principle of observable kinds. I also feel
inclined to say that this latter principle is very " fundamental "
for positivism, that it is in some way more fundamental and
more " important " for positivism than the positivist principle
itself. I even feel inclined to say that the principle of observable
kinds in some way expresses the inner " essence " of positivism,
and that, if positivists have never stated it themselves, they
ought to have done so, because it expresses a " deeper " principle
of their philosophy than they have ever themselves expressed,
because it is what they themselves have been groping for and
trying to say all along without succeeding. I feel inclined to
say that it expresses their " real " meaning. But unfortunately
I have to admit that I do not know what most of these things,
which I feel inclined to say, exactly mean. Yet having written
them down, as expressions of unclear ideas, I feel disinclined to
cross them out. For although I do not know exactly what
they mean, I believe that they mean something, and that this
something is probably true. I shall leave it at that. What I
shall go on to do is to try to show—the one point which I said
I did feel sure of—that the positivist principle logically implies
the principle of observable kinds, and that therefore the positivist
must accept it as a part of his philosophy, whether he has ever
previously formulated it or not, whether he has ever thought
of it or not, whether he thinks it important or not, and whether
he *wants* to accept it or not.

My belief is that the positivist has never explicitly formulated
the principle, but that it has always been, so to speak, " at the

back of his mind ", that he uses it as an unstated premise in his arguments, and that it influences all his thinking, whether he is himself aware of the fact or not. He *may* therefore, when it is explicitly stated, say, " of course, that is what I meant all the time. It is obvious and not worth stating." Or, on the other hand, he may not say this, or anything like it.

6.

I have to show that the positivist principle logically implies the principle of observable kinds. First of all, however, I will take a particular instance of a positivist contention and try to show that the philosopher who puts forward this contention has the principle of observable kinds " at the back of his mind ", that he is (perhaps unconsciously, perhaps consciously) relying upon it, that it is at any rate a necessary premise of his argument, although he nowhere explicitly states it, nor seems to recognize it.

In the first chapter of his book, *The Foundations of Empirical Knowledge*, Mr. Ayer considers the case of the philosopher who asserts that we only perceive sense-data, never material objects. Suppose this philosopher is in dispute with another philosopher. Philosopher A says, " We do not perceive the table or the chair, we only perceive sense-data which we believe to belong to a table and a chair ". Philosopher B says, " No, what we perceive is the actual table and the actual chair ". Mr. Ayer wishes to show that in such a case the two philosophers are not disagreeing about any question of fact, but only about a question of language. In order to prove this he uses the following argument. The two philosophers, he points out, may examine the objects before them—that is to say the objects which ordinary people would call tables and chairs—and they may never disagree about any observation. They will agree about the colour, shape, weight, about every fact which could possibly be observed. Since they agree about all possible observable facts, there is between them no disagreement about facts ; hence they can only be disagreeing about the language in which the facts should be described.

Now it is quite obvious that Mr. Ayer's argument as it stands is a *non-sequitur*. He argues :

The philosophers agree about all the observable facts.

Therefore they agree about all the facts.

But what if the philosophers should say that they are disagreeing about *un*observable facts ?

This, of course, may seem to a positivist to be absurd. I
shall contend in this article that it is not absurd, *i.e.*, that a state-
ment that an entity of a kind which it would be logically impossible
to observe exists may be a significant statement. Of course
such statements can be attacked on other grounds, for example,
by alleging that no inference from what is observed to what is
in principle unobservable can ever be a valid inference, and that
therefore we can never know whether such statements are true
or false. This is quite a different question, which I am not
discussing in this paper. It is quite possible that all the argu-
ments which philosophers have used to prove the existence of
unobservables are bad arguments. But my contention is that
their arguments have to be considered on their merits and
answered ; and that they cannot be put out of court without a
hearing on the ground that their conclusions are not even
significant statements. Meanwhile it is at any rate a fact that
philosophers A and B may themselves believe that they are
disagreeing about unobservable facts. For instance, philosopher
A may hold the view that there is a " physical object ", X, which
possesses intrinsic qualities which correspond to the perceived
qualities of shape, size, colour, smell, etc., but which are forever
hidden from us, so that we can never know anything about
these intrinsic qualities except the fact of their correspondence
to perceived qualities ; and that this physical object X stands
in a causal relation to our sense-data. (Some such view as this
was actually put forward by Russell in chapter 3 of his *Problems
of Philosophy*.) Philosopher B may deny that any such object
as X exists. He may say that the table or the chair simply *is*
the collection of all the sense-data which (according to A) are
caused by it. Thus both of them may admit the existence of
the sense-data, and may entirely agree about all their char-
acteristics, which means that they will agree about all the ob-
servable facts. But they will assert that they are in disagree-
ment whether X exists or not. X, if it is a fact, is a fact forever
unobservable. These philosophers will therefore say that they
are in disagreement about whether or not there exists an alleged
unobservable fact. Mr. Ayer *appears* to have overlooked this
in his argument.

But I do not think he really has overlooked it, nor that his
argument is really the *non-sequitur* which it appears to be. For
does it not contain an unexpressed premise ? The missing link
in the argument, the unexpressed premise, the premise which
the argument requires to make it valid, is the principle of ob-
servable kinds. The reason why he draws the conclusion that

the dispute between A and B is linguistic is that he assumes that
any statement about X, that it exists or does not exist, is non-
significant, because such a statement asserts or denies a fact
which is of a kind which it would be logically impossible directly
to observe. A may say that X exists, and B may say that
it does not. But neither of them are saying anything at all,
and therefore there is no factual disagreement between them.

I think that, in all probability, one may generalize from this
instance. I think that when positivistic philosophers say that
" philosophical propositions are verbal ", or that philosophical
disputes are never disputes about fact but always about the
way in which language should be used, or that they are " re-
commendations " about the use of language, they are practically
always relying, consciously or unconsciously, upon the principle
of observable kinds. If so, I shall perhaps be considered to some
extent justified in having suggested that the principle of observ-
able kinds is very " fundamental " and very " important " in
their philosophy, and that it expresses in some way a " deeper "
principle which " underlies " all their thoughts, whether they
themselves express it or not, whether they agree to it or not,
whether they know it or not.

7.

What the last section showed was that in some particular
instances of positivistic contentions, or at any rate in one
particular instance, positivism assumes and takes for granted
the principle of observable kinds ; that it requires this principle
as one of its premises. It is now desirable that I should try to
show, in general terms, and in a more rigorous way, that the
positivist principle logically implies the principle of observable
kinds, so that the positivist *must* admit it as part of his system.

Consider the situation P → Q, which the positivist has in
mind when he affirms the positivist principle. If it is *not* the
case that the positivist principle implies the principle of ob-
servable kinds, then it *might* be the case that although the facts
stated in Q are logically capable of being directly verified, P
itself might state facts of a kind which it would be logically
impossible to observe. And indeed for all the positivist principle
tells us, this might be the case. For it tells us that the facts
stated in Q must be observable, but it does not tell us anything
at all about the observability or unobservability of the facts
stated in P. Could it then be the case that the facts stated or
denied in P might be of such a kind that it would be logically

impossible to observe them ? When the matter is put in this way, I think the positivist will almost certainly reply that it could not be the case. But it is desirable to see why this is so. There are two cases.

(1) Where P → Q is a deductive argument.
(2) Where P → Q is a cause-effect inference.

Case (1).—If P → Q is a deductive argument, then it may be held either (*a*) that Q states the same facts as P, either in whole or in part, or (*b*) that Q states some facts or elements of fact which are not asserted in P. Sub-case (*a*) is the view that logical rules are rules of linguistic transformation. Sub-case (*b*) is the view that in the conclusion of a deductive argument there may be some element of fact which is " new ", *i.e.*, which is not " contained " in the premises.

If we accept the view embodied in sub-case (*a*), then it is clear that if Q states facts of a kind which it is logically possible to observe, then P must also state facts of a kind which it is logically possible to observe, since P and Q merely state the same facts in different terms. (It is true that theoretically Q might state only *some* of the facts which P states, and that the *other* facts stated in P might be unobservable. But on the transformation view of deduction these other facts could not be a necessary part of the premises.) Hence in this case the positivist principle implies the principle of observable kinds.

If we take the view of deduction involved in sub-case (*b*), then I have to admit that I do not at present know how rigorously to prove that if Q states observables P must state observables.[1] For according to this view the facts stated in P might conceivably be of a quite different kind from the facts stated in Q. I think it will be admitted that this is a very unplausible view. But for lack of a definite disproof I shall have to have recourse to an argument *ad hominem*. It is notorious that positivists take the view that logical deduction is linguistic transformation. Therefore it does not lie in their mouth to object that the facts stated in P might be of a wholly different kind from the facts stated in Q. As against them, accordingly, my proof is rigorous.

Case (2).—If P → Q is a cause-effect inference, it is certain that the facts stated in P cannot be unobservables if the facts stated in Q are observables. For the inference must rest on a causal law. The cause C will be the fact stated in P, while the

[1] I shall use the word " observable " hereafter to mean " a fact of a kind which it would be logically possible to observe " and the word " unobservable " to mean " a fact of a kind which it would be logically impossible to observe ".

15

effect E will be the fact stated in Q. For instance, P may state
the fact that it rained five minutes ago, while Q states an effect
of this rain, namely, that the ground will be wet now. But it
is impossible that the causal connection between C and E can
have been established except on the basis that E has been ob-
served to follow C. Therefore C, the fact stated in P, must be
an observable.

From these contentions it follows that the positivist principle
implies the principle of observable kinds. Hence if the principle
of observable kinds is false, it will follow that the positivist
principle must be false. We have therefore now to examine the
principle of observable kinds from the point of view of trying
to determine whether it is true or false.

8.

No one, I believe, will maintain that the principle of observable
kinds is " self-evident " (whatever that may mean).

9.

Is it, then, an arbitrarily adopted definition of significance, or
a stipulation ? Positivist philosophers do, I think, sometimes
talk as if their principles were merely definitions which they
choose to adopt. If this position were taken up, I should have
only to say that the definition of significance given in the prin-
ciple of observable kinds is not binding upon anyone who does
not choose to accept it—and that *I* do not choose to accept it.
Anyone has a right to define a bird as a species of five-legged
mastodon, but if he does so, the ornithologist, that is, the person
who is interested in real birds in the real world, will not pay
any attention to what he has to say about birds. Likewise
the positivist may define significance in any way he pleases, but
there is no reason why philosophers should pay any attention
to him unless he can show that his definition has empirical
application to real significances in the real world of discourse.
And this can only be done by an empirical investigation of
significant propositions, *i.e.*, through inductive generalization—
just as the generic characters of the genus bird can only be
discovered by an empirical survey of birds.

10.

It remains therefore to consider whether the principle of
observable kinds has the status of a properly framed inductive

generalization. And here again positivist writers do sometimes seem to hint that their principle has been inductively arrived at. For what else can be the point of telling us that their principle is based upon the actual procedures of science ? If this means anything, it must mean that scientific propositions (which have first been admitted to be significant) have been examined, and that the common element of significance in them all has been found to be that which is set forth in their principle—or at least it must mean that they believe their principle *could* be justified in this way. It is doubtful, however, whether this claim can be upheld by an actual *unbiased* survey of scientific propositions. For it is not at all certain that there are not highly respected scientific hypotheses which allege the existence of unobservables. It is at least arguable that the electron hypothesis is of this kind. It is at least arguable that scientists themselves attribute no observable characteristics whatever to the electron, whether in the way of primary or secondary qualities.[1] They certainly do not attribute secondary qualities to it. And as to primary qualities, it is doubtful whether the space-time of which they speak has any of the characters of perceived spaces and times. Of course positivists can and do *interpret* the electron hypothesis so as to *make* it conform to their principles. They can and do interpret it as a construction out of sense-data. And for all I know this may be the best way to interpret it. But can it be said that, if this is done, their survey of the empirical evidence (which consists of scientific theories) is an unbiased one ? For their procedure then is that they first so manipulate the evidence as to make it conform to the conclusions at which they wish to arrive ; and then arrive at their conclusions upon the basis of this manipulated evidence.

But suppose we admit, for the purposes of argument, that the evidence to which the positivist appeals, namely the body of admittedly scientific propositions, does support their conclusions. We must then ask why they have restricted their basis of evidence to *scientific* propositions. Can it be considered a sound inductive method first to exclude certain propositions, such as ethical and philosophical propositions, from the evidence, and to retain only scientific propositions ; and then on this basis to erect a definition of significance ; and then by means of this definition to rule out ethical and philosophical propositions as not having significance ? What would one say of an

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[1] I am not suggesting that the old distinction between primary and secondary qualities can be maintained. But it is still sometimes useful as a rough classification of the perceived characters of an object.

anthropologist who, seeking a definition of man, should first in-
clude only European men in his survey, should conclude that
whiteness of skin is one of the defining characteristics of man,
and should then proceed to tell us that *negroes are not men ?*

It might be urged that the cases are not wholly parallel. And
I should not wish to press the analogy between them too far.
But what I think these considerations do show is this : that the
positivist starts with a bias in favour of some kinds of proposi-
tions (scientific propositions) and against other kinds of proposi-
tions (philosophical propositions) ; and that in consequence he
cannot make and never has made any honest and impartial
empirical survey of significant propositions with a view to
discovering the nature of significance. He cannot therefore
claim that his principles are justified as genuine scientific
inductions.

Is there, however, no other kind of evidence to which he
appeals ? I think there is. I think it is almost certain that
positivists believe that their principles are a *development of the
general principle of empiricism.* They call themselves " empiri-
cists ". And thus by implication they claim that, whatever
evidence there is to support the general principle of empiricism
is also evidence which supports them. They think that, although
their position is in some way different from that of other em-
piricists (such as Hume)—more " advanced " no doubt—yet
it grows out of the same root as does the tree of empiricism,
and that therefore the sap which nourishes that tree will also
nourish them. This is a very interesting and also a very important
claim. And I propose to examine it. The question is : Is
positivism a legitimate development of empiricism, and are the
grounds which support empiricism also grounds which support
positivism ?

11.

There may be a number of different senses of " empiricism ",
in other words a number of doctrines or propositions which,
though differing from one another, have all been called em-
piricism. If so, the positivist who claims that his doctrine is a
legitimate development of empiricism ought to tell us which
kind of empiricism he is talking about. He ought to tell us in
what sense of empiricism he claims to be an empiricist. But
so far as I know no positivist has ever made any attempt to do
this. Now I cannot be expected to catalogue and distinguish
all the doctrines which have ever been called empiricism and
then to show that the principle of observable kinds is not a

legitimate development of any of them. And since I cannot do this, my argument, perhaps, cannot be quite complete. (The blame for this must lie at the door of the positivist, since it was for him, and not for us, to say what he means when he claims to be an empiricist.) I can, however, at least specify what seem to me to be the two main types of theory which have commonly been called empiricism, and can show that neither of them gives any support to the principle of observable kinds.

I think that the two main doctrines which have commonly been called empiricism are (1) the doctrine that all *knowledge* is " based upon " or " derived from " experience, and (2) the doctrine that all " *ideas* " are " based upon " or " derived from " experience.

Clearly the precise meanings of these two theories stand in much need of clarification. I cannot attempt any exact analysis of them here. But the following observations are relevant to the purpose of this paper. The meanings of the phrases " based upon " and " derived from " appear to be quite different in the two different cases. Ideas are not said to be " based upon " experience in at all the same sense as knowledge is said to be " based upon " experience. What appears to be meant by the statement, in the first theory, that all *knowledge* is " based upon " or " derived from " experience is that if any proposition is known to be true, it can only be so known because there is empirical evidence for it, in other words that the grounds of all knowledge must be empirical grounds. Thus Mill's view that the proposition $2 + 2 = 4$ is an empirical generalization illustrates this kind of empiricism.

On the other hand, when it is said, in the second theory, that *ideas* are " based upon " or " derived from " experience the meaning of these phrases must be quite different from that given above. For there is here no question of knowledge or truth or evidence. The idea of a centaur is " based upon " our experiences of men and horses, and ultimately upon our experiences of colours, shapes, etc. But the idea of a centaur is not as such true or false (in so far as no proposition is asserted about it), nor is it a case of knowledge. The sense in which, according to this doctrine, what Hume called simple ideas are based upon or derived from experience (Hume's " impressions ") seems to be something like the sense in which a copy of something may be said to be based upon or derived from its original. And when it is said that compound ideas, too, are based upon experience, this seems to mean that a compound idea can be analysed into a number of simple ideas which are based upon experience in this sense.

The first type of empiricism seems to be that which Russell has in mind when he speaks of certain principles of knowledge, such as the principle of induction, as being "limits" (exceptions ?) to empiricism. For this apparently refers to the fact that we cannot know the principle of induction to be true by virtue of empirical evidence.

The second type of empiricism is that which was maintained by Hume in, for example, the second section of his *Enquiry concerning Human Understanding*.

Now it can be shown in very short space, I think, that the principle of observable kinds does not follow from, and is not in any sense a legitimate development of, the first kind of empiricism. For the principle of observable kinds professes to be a criterion, not of the *truth* of propositions, nor of ways of *knowing* them to be true, but of whether they have *significance*. But the first kind of empiricism has nothing to do with significance at all, and cannot so far as I can see have any bearing on that subject. What it tells us is that the only way of knowing a proposition to be true is through empirical evidence. And thus what it professes to provide is a criterion for distinguishing between propositions which can be known to be true and propositions regarding which we cannot know whether they are true or false. It is, however, impossible that a doctrine which is a criterion for distinguishing what is significant from what is non-significant could ever follow from, or be a legitimate development of, a doctrine which is a criterion for distinguishing what is known to be true from what is not known to be true. For whatever the relations between "being known to be true" and "being significant" may be, they are certainly not the *same* thing, since a proposition may be significant and yet not known to be true. A significant proposition may in fact be known to be false. The long and short of it is that the first kind of empiricism is a theory about the *truth* of propositions (more correctly about how their truth can be known) while the principle of observable kinds is a theory about the *significance* of propositions. And since the two theories are "about" different subjects, one cannot possibly follow from, or be legitimately developed out of, the other.

Before leaving this branch of the subject I ought perhaps to say that there is one opinion which is very often held by positivists and which *can* reasonably be described as a development of the first kind of empiricism. And confusion is likely to occur unless it is pointed out that this opinion, although it happens that it is held by many positivists, in addition to their positivism,

is not itself positivism (as here defined) ; so that from the fact that this opinion is a development of the first kind of empiricism it does not follow (and it is not true) that positivism is a development of the first kind of empiricism. The opinion to which I refer is that all *a priori* propositions are analytic. This does seem to follow from the doctrine that all knowledge of matters of fact must be based on empirical evidence. But positivism as here defined means the positivist principle and/or the principle of observable kinds. And the point of this section was to prove that *these* are not legitimate developments of the first kind of empiricism, and do not follow from it.

12.

It remains to be considered whether the principle of observable kinds is a legitimate development of the second kind of empiricism. And here let me say—though it is not strictly relevant to my argument—that this is what, I should imagine, the positivist must (very confusedly) think when he claims to be an empiricist. For the claim that the positivist principle is a development of the second kind of empiricism is at least more plausible than the claim that it is a development of the first kind of empiricism. The second kind of empiricism does at any rate deal with meaning (as Hume himself clearly recognized) [1] and not with truth or knowledge. It is a criterion for distinguishing the meaningful from the meaningless. It might therefore be supposed that it would be possible to derive from it a doctrine of the significance of sentences. For significance is a kind of meaning. Actually any such transition from the second kind of empiricism to a doctrine of the significance of sentences is impossible because the subject of the second kind of empiricism is meaning in the sense of the meaning of a word, and not meaning in the sense of the significance of a sentence. And these are quite different things. But to suppose that such a transition is possible is at least a natural and understandable mistake, since both the second kind of empiricism and the positivist doctrine of significance are concerned with meanings (though in different senses). And this is why I am inclined to think that this is what the positivist who claims that his theory is a development of empiricism must mean. He is very likely

[1] " When we entertain, therefore, any suspicion that a philosophical term is employed without any meaning or idea . . . we need but enquire from what impression is that supposed idea derived."—*Enquiry*, Second Section.

I think, to have made this natural and understandable mistake ; whereas he is much less likely to have made the palpable and inexcusable blunder of thinking that his doctrine of *significance* can be developed out of a doctrine about the grounds of *knowledge*. But of course it does not really matter whether I am right about this or not. All I have to show is that, whichever of these two mistakes the positivist has made, they both of them *are* mistakes. I have shown in the previous section that the principle of observable kinds cannot be a legitimate development of the first kind of empiricism. And if I can now show that it cannot be a legitimate development of the second kind of empiricism either, my task will be complete. I will now try to show this.

The principle of empiricism as explained by Hume is simply this : that the mind cannot spontaneously generate " simple ideas ", nor create them out of nothing, but has to derive them from what Hume called " impressions ". By simple ideas he meant the ideas of unanalysable qualia, such as hotness, redness, softness, hardness, sound- and smell-qualia, etc. The essence of the principle is simply : *no impressions, then no simple ideas.* Once you have gathered the simple ideas from experience, *i.e.*, from impressions, you can build up compound ideas out of them in various ways. You can build houses if you have the stones ; and subject to certain principles, such as the laws of gravity, you can arrange your stones in any patterns you please. But you cannot create your stones out of nothing ; you have to get them from the quarry. Likewise you can build compound ideas out of simple ideas and, subject to certain principles, such as the laws of logic, you can arrange the simple ideas in any patterns you please. But you cannot create simple ideas out of nothing. You have to get them from the quarry of direct experience.

Hume's language is ambiguous and faulty, especially in regard to the terms " idea " and " impression ". It is not difficult to rid it of at least the more serious ambiguities, and to restate the principle in more exact terms. Elsewhere I have tried to do this,[1] but in this paper I shall stick to Hume's own terms, since the reader is familiar with them, and since their ambiguities will not affect the argument of this article.

It should be noted that Hume recognized his principle as an empirical generalization and carefully indicated the evidence on which he believed it to rest. He tells us that it is proved by two arguments. The first is that one can always analyse a genuine compound idea into its component simple ideas and then

[1] *The Nature of the World*, p. 10.

point out the impressions from which the latter are derived.
Under this head he does not commit the mistake of restricting
his evidence to scientific ideas. On the contrary, one of his first
examples is the idea of God. This, he tells us, is based upon
such impressions as those of wisdom and goodness which we find
in our experience of ourselves or other men, and the ideas of
which we then " augment " without limit to form the idea of
an infinitely wise and good being. The second argument is that
man born blind cannot frame the idea of colour, that men born
deaf cannot frame the idea of sound, etc. I cannot, of course,
discuss this evidence in detail here. I believe that it is, in
general, sound and affords satisfactory inductive proof of the
principle of empiricism. The question with which we are con-
cerned is : does this principle support the principle of observable
kinds ? Or can the principle of observable kinds be regarded
as in any sense a logical development of the principle of
empiricism ?

The first point to notice is that the principle of empiricism
gives no directions as to *how* simple ideas are to be combined
or compounded into complex ideas. *It provides no rules of
combination.* No doubt such rules exist and can be stated.
But they are not part of the principle of empiricism, do not
follow from it, and are logically independent of it. Some of
the rules may be mentioned. First, there are the laws of logic
which forbid certain combinations of ideas. For instance, one
cannot speak of a square circle. Secondly, there is what I will
call the *Principle of Incompatibles*. This states that incompatible
characters cannot be combined in the mode of spatio-temporal
coincidence, though they can in the mode of spatial juxtaposition.
Thus the same surface cannot be simultaneously red all over
and blue all over, though red and blue can exist simultaneously
if juxtaposed. Thirdly, there are the rules of syntax which forbid
as non-significant such combinations of ideas in a sentence as
" either is blueness sideways ".[1] If one were trying to make
a complete list, it is possible that one ought to add certain rules
arising out of the theory of types. But the point on which I
wish to insist here is that such rules do not form any part of the
principle of empiricism, and cannot in any way be derived from
it. The principle of empiricism concerns only the origination
of simple ideas, nothing else. It tells us : no impressions, then
no simple ideas. We may add as part of the principle, if we
wish, the fact that certain of our ideas are not simple but are

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[1] As a matter of terminology, I do not include the laws of logic under
the rules of syntax.

compounded out of simple ideas. But this is absolutely all that the evidence on which the principle rests will support. It does not support any statement about what combinations of simple ideas are allowable and what are not. The rules of combination, whatever they are, do not rest upon the evidence on which the principle of empiricism rests ; they cannot be derived or developed out of the principle of empiricism ; and they are wholly independent of it.

I do not see how it can be denied that the rules for the combination of ideas are logically independent of the principle of empiricism. Obviously the principle " no impressions, then no simple ideas " does not logically imply the laws of logic, nor the principle of incompatibles, nor the rules of syntax. Nor will the further statement that some of our ideas are compounded out of simple ideas logically imply these principles. And this conclusion follows, not only in regard to the particular rules of combination which I have mentioned, but to any rules whatsoever. For it is clear that no rule of any kind can be derived logically from the principle of empiricism as above stated. I conclude that the principle of empiricism cannot forbid any combination of ideas whatever, nor tell us anything at all about how they ought or ought not to be combined.

The next point is that a sentence always expresses a complex of ideas, never a simple idea. The linguistic sign for a simple idea is, I believe, always a single word or at most a phrase. Such words as " red ", " hot ", express simple ideas. A sentence expresses the complex fact of A-being-B or A-being-related-to-B. Thus if F is the fact asserted or denied in a sentence, then F = $abcd$ . . ., where a, b, c, etc., are the simple constituents of F. Hence the " idea " which the sentence conveys is always a complex idea.

Since all rules about how to combine ideas are logically independent of the principle of empiricism, it follows that neither the principle of empiricism nor any principle which follows from it can ever forbid any combination of ideas in a sentence. But the principle of observable kinds is a principle which purports to forbid certain combinations of ideas in sentences. For instance, the sentence " There exists a physical object which could never be observed nor its intrinsic qualities known but which stands in a causal relation to our sense-data " is a complex of a number of ideas such as " object ", " quality ", " not ", " knowable ", " observation ", " cause ", " sense-data ", etc. Each of these separate ideas, we presume, can be traced back to its basic impressions. It is not alleged by the positivist

that any of the words used are meaningless words. But the principle of observable kinds tells us that we cannot combine these ideas in the way in which they are combined in this sentence, or that if we do attempt so to combine them the sentence will be non-significant. But I have proved that no such rule, directing us how to combine or not to combine ideas, can ever follow from the principle of empiricism. Therefore the principle of observable kinds cannot follow from it. This means that the principle of observable kinds cannot claim to be a development of empiricism. It cannot claim that it is supported by any of the grounds which exist for thinking that empiricism is true. It has nothing to do with empiricism. And this means that positivism is not a legitimate development of empiricism, and that it has in fact nothing to do with empiricism.

In spite of this, of course, the principle of observable kinds might be true. But our grounds for thinking that it is true are now vastly diminished. It is not self-evident. It cannot claim truth as a definition or stipulation. It is doubtful whether the procedures of science—if taken as they appear in the sciences and not as manipulated by the positivist—support it. And even if they do, this basis of evidence, restricted as it is to scientific sentences, is far too narrow to yield a trustworthy generalization about the significance of *all* sentences. Finally, the claim that the principle of observable kinds is a development of empiricism, and that whatever reason there is for believing in empiricism is also a reason for believing in it, has been disproved.

But I think we can go further than this. There is every reason to suspect that the principle of observable kinds is false. For the foregoing analysis of its relation to empiricism brings to light the fundamental blunder which the positivist has committed. He has failed to keep before his mind the distinction between the meaning of a word and the significance of a sentence. The principle of empiricism is a rule concerning the meanings of words. It has nothing at all to do with the significances of sentences. But the positivist has tried to apply the principle to the question of the significances of sentences.[1]

Consider the sentence " the table is to the left of the chair ". What can the principle of empiricism tell us about this ? It can take the distinguishable ideas which c ompose the proposition,

[1] Russell has made this criticism of Schlick's original principle of verifiability (see *Enquiry into Meaning and Truth*, p. 386). My point is that the same criticism really applies to the positivist principle in its modern form.

such as " chair ", " table ", " to the left of ", trace them back
to their impressions, and so vouch for the genuineness of the
ideas and the meaningfulness of the words which symbolize
them. What it tells us is that if a sentence asserts or denies a
fact F, which is a complex of $a, b, c, d, \ldots$, then each of these
simples, $a, b, c, d, \ldots$, must be an observable. But what the
principle of observable kinds does is to assert that the total
complex fact F, or $abcd$, must *as a whole*, be an observable.   But
for this there is not the slightest warrant in the principle of
empiricism. And there is every reason to think that it is false.
For this assertion has clearly arisen from trying to apply to the
total complex which is the significance of a sentence the condition
of observability which the principle of empiricism asserts must
apply to the constituent simples which are the meanings of words.

13.

Strictly speaking my argument ends here. I will, however,
add a few words regarding a question which will inevitably
be asked at this point. If the principle of empiricism yields
no criterion for deciding what sentences have significance, and
if the positivist principle and the principle of observable kinds
are rejected as such criteria, then what are the true criteria ?

The criteria will obviously consist in rules concerning the
combinations of ideas. They will tell us in what ways ideas
can be combined in sentences and in what ways they cannot.
Certain rules of combination have already been suggested, *viz.*,
the laws of logic, the principle of incompatibles, and the rules of
syntax. Are these the criteria of which we are in search ?

In my opinion the laws of logic are not criteria of whether a
sentence is significant. They are criteria of whether it is true.
Thus the law of contradiction tells us that the sentences " x is
a square " and " x is a circle " cannot both be *true*. Therefore
it tells us that the compound sentence " x is square and x is cir-
cular " or " x is a square circle " is *false*. It does not, as many
philosophers think, tell us that this sentence is non-significant.
How can it tell us anything about the question of the *significance*
of the compound sentence when what it told us about the two
simple sentences taken separately concerned their *truth ?* How
can the junction of the two sentences by the word " and " make
this difference ?

The same argument applies to the principle of incompatibles.
Hence " x is red and green all over " is false, not non-significant.

This leaves us only with the rules of syntax as criteria of

significance. Hence, in my opinion, the procedure for dis-
covering whether a sentence is significant will contain two steps.
First, it must be ascertained by means of the principle of em-
piricism whether the separate *words* (or whatever linguistic
symbols are used to stand for single distinguishable ideas) have
meaning. (For of course the principle of empiricism has this
much to do with significance—that if a sentence is composed of
meaningless words it cannot be a significant sentence.) Secondly,
if all the words are meaningful, we have then only to see whether
they are combined according to the rules of syntax. If they
are, the sentence has significance. If not, not.

By these rules " either is blueness sideways " will be rejected
as non-significant. But many sentences which positivists, as
well perhaps as some other philosophers, would reject as non-
significant will be seen to be significant. Under this heading
will come not only " this is a square circle ", " red is blue ",
" virtue is to the left of stupefaction " (all false), " quadratic
equations do not go to horse races " (true),[1] but also " there
exists a physical object which it is logically impossible to observe,
having intrinsic qualities which we can never perceive, and which
is causally related to our sense-data " and most of the famous
" metaphysical " and philosophical sentences written by philos-
ophers in the past.

I have not, of course, discussed in this paper the question
whether any such philosophical sentences are ever true. Nor
have I discussed whether we can ever have any means of knowing
whether they are true or not. I have discussed solely the question
whether they can have significance.

[1] This is an example of Mr. A. C. Ewing's.

III.—PURE SEMANTICS, SENTENCES, AND PROPOSITIONS.

By Gustav Bergmann.

THE first four sections of this paper contain what is essentially an exposition of the nature and function of pure semantics as it has been developed in Carnap's recent *Introduction to Semantics* (1942). This presentation is not only entirely non-technical but also, from the standpoint of the formal logician, extremely sketchy and, worst of all, very unprecise. The precision here striven for is of a different kind, namely, philosophical explicitness and clarity. One of my aims is therefore to formulate, in a manner which is freed from all merely technical literal-mindedness, the epistemological idea and significance of pure semantics.[1] Such undertaking is not quite superfluous, as one distinguished reviewer [2] has already expressed the opinion that pure semantics deals merely with topics usually regarded as belonging to formal logic and is therefore somewhat lacking in philosophical import. This is certainly not the case if what is here taken to be the idea and function of pure semantics should turn out to be correct. But I also want to state as clearly as possible that I believe this idea to be unmistakably contained in Carnap's book itself, though somewhat buried under the weight of the necessary formal developments and, moreover, impaired by what I consider a misleading terminology and certain ambiguities in some statements of the English text. The fifth section elaborates the criticism which the last remarks imply ; but this criticism, let that be said again, does not attach to the core and substance of the work. The last section summarizes, in broad strokes, the epistemological significance of pure semantics by relating its contribution to more traditional formulations. In conclusion some suggestions are offered about the nature and function of epistemological pragmatics. These, however, are mere hints which stand in dire need of future elaboration.

I.

First it must be indicated how the term ' language ' will be used in this paper. Naturally no precise or exhaustive definition

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[1] Some such indications have been given, in a brief German paper, by Tarski. See *Actes du Congr. Internat. de Phil. Scient.*, Paris, 1935, **3**, 1-8.
[2] *Journal of Philosophy*, 1942, **39**, 468-472.

can be offered, but the following remarks will suffice to establish
our meaning for everybody who is somewhat familiar with
recent developments.   The building stones of a language, its
signs or *symbols*, are the elements of a given set.   As a rule, this
set is divided into exclusive subsets.   According to which of
these subsets they belong to, the signs are distinguished as
calculational constants, particulars or names properly so called,
predicates of the different degrees and levels, propositional
symbols, variables, etc., and they are all effectively recognizable
as such.   There is also a set of rules or definitions which deter-
mine which expressions, that is, which series of signs, are con-
sidered as the *sentences* of the language.   In the more interesting
cases that have been studied so far, the set of symbols is usually
infinite and the sentential series are finite.   A further set of
rules or definitions distinguishes a subclass of the sentences as
the so-called *theorems* of the language.   Throughout this paper,
the synonymous expressions ' sign ' and ' symbol ' are under-
stood to mean what is usually meant by types, not tokens.   If
we had started from tokens instead, no essential point would
have to be altered in the following discussion, only some tedious
verbalism would have to be added.   Languages more properly
so called, that is, languages which are not of merely mathematical
interest, have the following two properties : *first*, upon inter-
pretation they become at least partial schemata of the empirical
language, and, *second*, their theorems then correspond to state-
ments which, out of philosophical preconceptions, we call
analytic.   It is one of the main achievements of Carnap's
earlier work, as presented in his *Logical Syntax of Language*
(1934, 1937), to have given for the first time a set of rules which
exhausts our preconceived notion of analyticity.   By empirical
language I mean one of those informal, universal, interpreted
languages, say English, which we all understand and speak about
the world in.   What is meant by interpretation will be taken
up presently.

There has been a feeling among philosophers, recently voiced
by Ducasse,[1] that ' sign ', ' symbol ', and ' language ' are not
very apposite terms as long as interpretation or reference does
not enter the picture and the term ' discursive entity ' has been
suggested instead of ' sign '.   Since such criticism is essentially
a warning against confusions which could be caused by the
habitual connotations of the words in question, the point should
be readily conceded.   Probably the terms ' calculus ' or ' dis-
cursive universe ' are preferable to ' language '.   In the present

[1] *Philosophy and Phenomenological Research*, 1942, 3, 43-52.

discussion, at any rate, these three terms will be used syn-
onymously ; languages as such do therefore not refer to, or speak
about, anything.   To endow them with reference or, to say the
same thing differently, to make them languages in Ducasse's
sense, relations must be established between at least some of
their symbols and expressions on the one hand and what we
shall call *extralinguistic referents* on the other.   To the establish-
ment of such relations we shall refer as *interpretation*, to its result
as an interpreted language or calculus.   Though the possibility
of interpreting a given calculus by means of a given extralinguistic
universe depends in a complicated manner upon structural
properties of the calculus, interpretation is undoubtedly and in
an obvious sense extraneous to the calculus itself.

The technique of interpretation can be used to make the
expressions of one calculus the extralinguistic referents of another.
The interpreting calculus is then customarily referred to as the
object language, L, the interpreted one as the metalanguage, M.
' Extralinguistic ' does therefore not necessarily mean ' extra-
symbolic ', it is rather a term relative to a given universe of
discourse.   For ' extrasymbolic ' we shall presently introduce
the term ' nonformal '.   But it is of course also possible to
express calculationally and, as we shall see, even formally, the
relationship between an object language and *its* extralinguistic
referents.   Naturally, the metalanguage will in this case contain
symbols co-ordinated to, or, as one also says, names of, not only
the symbols of the object calculus, but also names for their
referents and for the referential relation itself.   Clearly, the
word ' name ' has just been used in the rather loose sense which
is customary in informal discussion.   The only technical sense
in which ' name ' occurs in this paper is that of ' particular '
or ' proper name ' as referring to certain symbols of a calculus
which are given and effectively recognizable as such without
any reference to interpretation.

A metalanguage is thus always an interpreted calculus, no
matter whether it is only about an object calculus or about
both an object calculus and its referents.   The important
difference is that only in the first case are we sure to stay within
the universe of symbols, though of course not within a single
discursive universe.   But in either case the interpretation of
the metalanguage is as extraneous to it as the extralinguistic
referents of the object language are to the latter.   To express
calculationally this interpretation or, as one usually says, what
the metacalculus " speaks about ", a further metacalculus
would be needed.   This regress is necessarily infinite, since no

discursive universe can conceivably exhibit its own referents, not even in those cases where some of these referents are to be found among its own symbols. It might be mentioned in passing that the status of such phrases as " I know that I know that I know, etc.", which ever since Descartes and Hobbes have played such a signal role in philosophical discussion, is closely related both to the possibility and to the inavoidability of this infinite regress. The principle of this regress, which is one of the corner-stones of Wittgenstein's thought, seems to me as ineluctable to-day as ever before. What the "last" calculus is about merely shows itself. The progress which has since been made can be characterized by saying that by means of the " preceding " calculi one is able to analyse many relationships which Wittgen-stein thought to be strictly unanalysable, among them the inter-pretational or semantical relationship itself. Whether the progress of such analysis is described as a destruction or as a reconstruction, as a solution or as a dissolution of the traditional philosophical problems is a question of taste and historical perspective.

To these general issues we shall return later ; let us conclude the present section with another terminological agreement. If a metalanguage refers only to an object language and not to its reference, we shall say that the metacalculus is *formal*, or that the object calculus has been dealt with *formally*. ' Symbol ', ' sentence ', ' theorem ', for instance, are predicates which can occur in a formal metalanguage.   On the other hand, the name of the relation which obtains between expressions of the object language and their extralinguistic referents cannot occur in a formal metalanguage. As is well known, the customary name of this latter relation is ' designation ' ; it occurs in such sentences of M as ' b designates a ', where ' b ' refers to the symbol of the object calculus and ' a ' to its referent. Frequently, one writes either ' ' a ' ' or a Gothic letter, ' $\mathfrak{a}$ ', instead of ' b '.

II.

In languages more properly so called, a formal distinction can be made, without any reference to interpretation, between *descriptive* symbols on the one hand and *nondescriptive* or logical symbols on the other. Its fundamental significance lies in the circumstance that every interpreted calculus must contain descriptive symbols. Calculi without descriptive symbols are called logical. ' Logical ', one sees, is not synonymous with ' formal '. At this point an objection is likely to arise in the

16

mind of the reader. If every metacalculus, as has just been asserted, is an interpreted calculus, then no metacalculus can be a logical calculus. But is not arithmetic, which can be constructed as a purely logical calculus and has been so constructed in the *Principia Mathematica*, also considered to be the universal syntactical metalanguage ? [1] The way out of this apparent impasse is simple enough. Syntactical studies, such as Goedel's famous contribution, neglect to formalize, or, as one should rather say, to express calculationally, one step which is indeed quite trivial and negligible within the syntactical context. This step is nothing else but arithmetization itself, that is, the co-ordination of the expressions of the uninterpreted object language, L, to numbers of the so-called arithmetical metalanguage, A. If one wants to proceed calculationally, a proper metalanguage, M, is needed and then a further one, G, which co-ordinates M to the arithmetical calculus A. Even so we would not yet have calculationally expressed the (semantical) relationship between L and M. One could of course use as M a quasi-arithmetical calculus whose integers are construed as particulars and made to serve as names of the expressions of L ; [1] but even in this case a further calculus, let us again call it G, is needed wherever the object language, as one usually says, contains (part of) its own syntax. For to express this one has to co-ordinate part of M to the arithmetical part of L. In making all these steps explicit we get at least rid of the paradoxical situation that the object calculus, which in syntax is thought of as uninterpreted, is said to speak about itself. For an uninterpreted calculus does not speak about anything.

Attention must now be called to the reasons why the suppression of these various G-calculi is justified, so that the customary elliptical way of speaking is indeed perfectly harmless. First, these G-calculi are all formal, that is, they establish relationships between different universes of discourse without ever leaving the universe of symbols. Second, those relationships which they establish are simple and rather trivial one-to-one correlations which, for want of a better term, we shall call isomorphisms or isomorphic translations : no harm should come from the use, without further definition, of these two expressions

[1] The word ' syntax ' will be discussed in the fifth section. Up to this point I rely upon its denotation as represented by the studies mentioned in the text.

[1] Language II of Carnap's *Logical Syntax of Language* is of this kind. That the integers be particulars is not absolutely necessary, they could all be constant predicates of the same level. This qualification which, however, is rather irrelevant for our purposes, applies throughout.

in the present context. The omission of G is, furthermore, materially irrelevant because what the syntacticist is interested in is, that to the extent to which syntactical theorems do have " isomorphs " in the object language, these isomorphs are themselves analytic. ' Analytic ' is here a predicate of the first metalanguage M, but if a second metalanguage, MM, speaking about M, is added and if the process is iterated, this self-containedness of analyticality still holds true. This is indeed part of the formal justification of our preconceived distinction between analytic and synthetic. To say the same thing in a slightly different manner, the syntactical vindication of our notion of analyticality takes place in three steps. First, we show that ' analytic in L ' can be defined in a formal metalanguage M ; second, we show that an M-statement about the analyticality of an L-statement is, according to MM, either analytic or contradictory in M, and so forth ; third, we exhibit, by means of a series of trivial isomorphisms between the successive levels, that self-containedness which one could aphoristically express by : once analytic, always analytic. Later on it will be seen that the complementary part of the formal vindication of analyticality lies within pure semantics. It will also be noticed that the G-languages which we have used to establish these isomorphisms between any two levels of the hierarchy L, M, MM, etc., do not themselves belong to this hierarchy. In this hierarchy every level speaks only about the preceding one. Structurally, this is the clearest scheme of pure syntax ; technically, all kind of telescoping might or might not be possible.

The last observation provides the opportunity for a further remark which leads on to our main topic, the idea of pure semantics. Syntax, a field which for good reasons we have not yet defined, does study such relationships as isomorphisms between several calculi ; the syntactical character of such interlinguistic studies can always be recognized by their symmetry with respect to the object languages concerned. If, for instance, two languages, L_1 and L_2, are thus interrelated, ' sentence in L_1 ' and ' sentence in L_2 ' are both predicates of the relating metalanguage G. This should for the moment illustrate what is meant by the symmetry of the relationship, but we shall return to these matters later. Presently we shall see that the so-called semantical relationship between a language and its metalanguage is not of this symmetrical kind. It has already been pointed out that so far we have not yet calculationally expressed the relationship between an L and its syntactical M. The formalization of this relationship is indeed a part of

pure semantics, but again, it is its trivial part, which will be formulated later on by means of an obvious and formal G.

Part of what has just been said can be profitably restated by anticipating another objection which concerns itself with the use of the term ' formal '.   The G-language which has been used in syntax, for instance, has been called formal, though it inter-relates two calculi one of which, M, is moreover itself interpreted. Is there any fundamental, not merely technical, difference between such co-ordinations and those between symbols and extra-symbolic referents ?   To this one can but answer that here the level of our basic or, if you please, philosophical pre-conceptions has been reached.   The idea is that as long as we remain within the universe of symbols, though not necessarily within one universe of discourse, certain problems simply do not arise.   Somebody could of course hold that no metalanguage is really formal, since it always speaks about something, even if it speaks merely about symbols, and that therefore our whole conception of ' formal ' is spurious or, at least, pregnant with metaphysical presuppositions.   To this I know no answer. Only it seems to me that he who remains unsatisfied at this point is not unlike that Indian whom Locke expects to go on questioning who supports the turtle.   In other words, if one does not worry about presuppositions and takes ' formal ' in the sense in which it is here used, one has found the starting point for a comprehensive analysis of the whole of our experience.   In the course of this analysis one will also reach all the assurance which is obtainable that nothing has been missed because one did not stop for an investigation of those alleged presuppositions at the very outset.   After all, a beginning must be made some-where.   To borrow again from Locke's simile, the philosophical organization of our experience does not yield a pyramid but a sphere of mutually supporting segments, closed and resting in itself.   If it were otherwise, who would support the turtle ?

III.

We are now ready to explain what is meant by semantics or, rather, by pure semantics, for there is no need for dealing here once again with semantics as the term is usually understood. One reference, instead of many, to Charles W. Morris' well-known monograph, *Foundations of the Theory of Signs*, will suffice. At the end of the first section there was already occasion to mention metalanguages which are not formal since what they

refer to are both languages and their extralinguistic referents. They can always be recognized by the occurrence of the relational predicates of designation.   The one domain of these predicates consists of the names of the expressions of the object language, the other domain, of the metalinguistic names of the referents of those expressions ; in other words, both language and meta-language speak partly about the same referents.   The predicates 'true' and 'false' which also characteristically occur in such metalanguages M are applied to the names of sentences of the object language L.   A point to note is that my last two state-ments, concerning designation and truth, are not sentences of M, but sentences about M, belonging to a further metalanguage, MM, which, in the present case, is colloquial English.   In what follows, however, we shall by MM always mean a formal meta-language, which speaks about M only and is therefore entirely innocent of what M is speaking about.   The hierarchy L, M, MM, etc., is thus analogous to the one we have considered in syntax, the only difference being that the M which is now con-sidered does not speak about L only.   Formal and materially trivial G-languages can again be used to mediate within this hierarchy, but the use of any nonformal G would defeat the very purpose of pure semantics.   This purpose, as will be seen presently, is to determine how much about the interpretational relationship can be learned from a *formal* study *of* and *within* the hierarchy L, M, MM, itself.   And every G which speaks about the referents of L is by definition non-formal.   It will, however, be best to postpone the correlation between L and M by means of a materially trivial, formal G, which has already been mentioned towards the end of the last section.   Let us first see what can be said about M, formally considered.

M contains certain symbols, such as 'symbol', 'sentence', 'Fido', ''Fido'', 'dog', and ''dog'' ; but nothing in M, except possibly its structure, indicates that 'symbol' is the universal name (predicate) of the names of the symbols of L, that 'sentence' is the universal name of the names of the sentences of L.   We do know, by merely looking at M, that 'Fido', ''Fido'', and ''dog'', are all three particulars or names in the technical sense and that 'dog' is a predicate of M.[1]   Again, we do not know, as long as we consider M formally,

[1] Obviously 'Fido' is here taken as a constant of the zero-level or name properly so called, and 'dog' as a predicate of the first level.   Any other example would do as well since particularity as here considered is a linguistic affair.   See also the preceding footnote and *Philosophy of Science*, 1942, **9**, 283-293.

that ''Fido'' and ''dog'' are the M-names of certain L-symbols and that 'Fido' and 'dog' are the M-names of the referents of those very L-symbols. Let us elaborate these points by means of two familiar illustrations. Take, for instance, the following sentence of M :

(1)   'Fido is a dog' is a sentence.

In the inset, 'sentence' is a predicate of M, while in the last line before the inset, in the phrase "the following sentence of M", 'sentence' is a predicate of MM, correctly applied to (1) and likewise to (2) below, after each of these statements has been surrounded by a pair of additional quotes. Wherever the need arises, these two meanings can be distinguished by writing 'sentence$_M$' and 'sentence$_{MM}$' respectively. Another important feature is blurred by the use of colloquial English in this very informal discussion. Instead of writing, in (1) 'Fido is a dog', one should write : 'Fido' 'is' '(a) dog'. More technically expressed, 'sentence$_M$' is a predicate of the first level and of indefinite degree which yields a correct sentence of M only by juxtaposition with a series of particulars of M, provided that these particulars name symbols of L and are taken in the right order. To elaborate the last point, let us consider Tarski's definition :

(2)   'p' is true if and only if p.

Here ''p'' is the name in M of a sentential variable of L; it is therefore a particular like ''dog'', 'Fido', and ''Fido''. Accordingly, ''Fido' is a sentence', ''dog' is a sentence', 'Fido is true if and only if p' and the remaining three possible substitutions all yield *correctly formed*, though of course *incorrect*, sentences of M. 'Dog is a sentence' or 'dog is true if and only if p', on the other hand, are nonsentential expressions of M, because they juxtapose, in violation of the type rule for M (formulated in MM), 'true' and 'dog', or 'sentence' and 'dog', which are all predicates of the first level. Finally, 'p' which occurs at the end of (2), is a sentence of M just like (1) and (2) themselves. 'Sentence of M', it will be noticed, is but a colloquial transcription of 'sentence$_{MM}$'.

Metalanguages of the kind that has been discussed in this section are called *semantical metalanguages*. Pure semantics is the study of such metalanguages formally considered. In other words, pure semantics is the construction of a semantical metalanguage M, disregarding its interpretation, by means of a further metalanguage MM. A semantical metalanguage thus formally

considered, is called a *semantical system*, S. As in syntax,
colloquial English, supplemented by a few symbolic devices,
can be used as MM. S, however, is always a symbolic structure
in the strict mathematical sense of calculus ; as a matter of fact,
pure semantics *is* essentially the formalistic construction of such
a calculus. This definition raises a question and a problem.
The question is rather obvious : What is the point of pure
semantics thus defined ? We shall postpone the answer. The
problem is, how one can speak about a calculus as a semantical
system if one has agreed to disregard its interpretation, since it
is exactly a certain interpretation which makes a calculus a
semantical metalanguage or, for that matter, a metalanguage of
any kind. This puzzle, however, is easily dissolved : all one
needs to do in order to eliminate it even verbally is to replace
" interpretation " by " possibility of interpretation ". The
situation exhibits a restricted similarity with axiomatic or pure
geometry. What one does is to construct an uninterpreted
calculus, S, in which such *words* as ' true ', ' false ', ' designates ',
etc., the so-called semantical predicates, occur and which
contains, besides, certain sentences, called the semantical
theorems, which are the abstracts, isomorphs, schemata, which-
ever of these expressions one prefers, of the semantical theorems
of an interpreted metalanguage M. Like an axiomatic geometry,
S, too, is a descriptive calculus, but at a decisive point the analogy
breaks down. While the basic geometrical predicates are un-
defined descriptive predicates and the geometrical theorems
synthetic, the semantical predicates turn out to be definable
and the semantical theorems to be analytic (in M). This is,
for the philosopher, the main result of pure semantics and it
also provides the answer to the question which we have post-
poned. Empiricist philosophers have always felt, and more or
less vaguely and incongruously expressed, that the semantical
predicates are formal or linguistic concepts and that the semantical
theorems are, as one sometimes says, true by definition. To say
the same thing differently, it was felt that semantical statements
are as void of factual import as the so-called truths of logic and
mathematics. This claim has now been made good by construct-
ing, *within the universe of symbols*, a calculus S which is an
abstract of an empirical M, and by obtaining, through a formal
study of it, the two results which have been mentioned before,
namely, first, that the semantical predicates are definable, and,
second, that the semantical theorems, such as that every syn-
thetic sentence is either true or false, are analytic. And it is
also obvious that only if these results are reached in a strictly

formal manner, that is, without any consideration of the inter-
pretation of M, do they have the epistemological significance
which is claimed for them.   For to be formal or linguistic means,
as we now clearly see, to. be contained within the universe of
symbols.   Some philosophers, of course, could insist that the
decisive interpretation, to which I have here alluded by calling
S the abstract of an empirical M, is still the crux of the matter.
Again I know no answer other than that the last interpretation
is always strictly ineffable.   In this amended form Wittgenstein's
fundamental insight has not been invalidated by semantics
nor, for that matter, by any other development.   I, for one,
still consider it as the only alternative to either a realistic or
an idealistic ontology, if one does not want to follow the lead of
some pragmatists and cavalierly dismiss the fundamental issues.

It might not be inappropriate to conclude this section with
a restatement of the last points from a slightly different angle.
There is no subject matter which cannot be dealt with formally.
As shown by the classical example of geometry, this is done,
simply enough, by dealing formally with the language in which
the subject matter is treated.   Pure semantics is in this respect
no exception and what makes it significant is indeed not that
it *is* a formal treatment of M, but rather that *by means* of such a
formal treatment of M one can obtain the results which have
been mentioned already twice.   In this formulation, however,
the clause " by means of a *formal* treatment " is as essential
as the rest.   I know of no better way to epitomize this situation
than to insist that pure semantics does not deal with the *extra-
linguistic* referents, the designata, in the *extrasymbolic* sense
in which one usually understands these two terms.   It will be
well to remember, for future reference, that if these designata
are of a certain kind, ' proposition ' and ' state of affairs ' are the
customary expressions which are used instead of ' designatum '.

IV.

This brief section is largely an elaboration and, to a minor
extent, an amplification of the preceding one.   It has been seen
that in order to achieve its aim pure semantics must deal
formally with M.   One can therefore not very well start the
construction of S, which is this formal treatment, with a symbolic
inventory that distinguishes already among the particulars of S
that special class which we informally refer to as the names of
the symbols of L.   It is likewise not permissible to anticipate

the designation relation by starting out with a symbolic equipment like this :

<p style="text-align:center">Fido, ' Fido ', dog, ' dog ', etc.</p>

That all symbols which, in a typographical sense, are compounded by means of quotes are particulars has already been emphasized ; thus they are obviously indivisible symbolic elements, not compounded expressions. As W. V. Quine put it, trying to obtain a new symbol from a " quoted " one by omitting the quotes is as reasonable as an attempt to obtain again English words by cutting off the first and last letters of English words. Quotes are, strictly speaking, not among the symbols of S. To start with corresponding Gothic and Latin lettering is of course but another way of introducing quotes. Accordingly pure semantics begins the execution of its programme by defining the designation relationship, usually by means of MM, since the domains of this relationship are in most interesting cases infinite. Those particulars of S which by virtue of this definition designate something, may then be called names, whether this something is itself a particular, a predicate, or a compounded expression of S. Later on one can of course occasionally use quotes or Gothic lettering, whenever these devices are not likely to create confusions. To extend, as Carnap does, the Gothic lettering to the semantical predicates of S has the disadvantage that it typographically unifies two very different things, namely, that subclass of the particulars of S which one might call the *picture* of L in S, and those predicates of S which are correctly applied to members or series of members of this subclass. At this point the reader might find it helpful to reflect on the following statement : ' Predicate ' is a predicate (of S) which applies by definition to those particulars (of S) which, by virtue of the definition of designation, designate predicates (of S). This is of course a statement of MM, which abbreviation we have retained for the metalanguage which deals formally with the semantical system S.

Naturally the construction of S is not finished, but has hardly been started by laying down the rules of designation ; technically it is not even necessary to begin in this manner, though it is undoubtedly the one which is most conducive to a general understanding. The next step of prime significance would then be the definition of truth which, in some way or other, is modelled after Tarski's original suggestion ; and so forth. But all this can be safely left to the mathematical expert. So we can turn to our next point.

It will have been noticed that I just avoided the term 'isomorphism' and spoke of the picture of L in S instead. In speaking about this relationship we are using that formal auxiliary G which has already been mentioned several times. A sufficiently rich MM could be used as G, but such technical economy would blur the clear outlines of the semantical structure. Materially the L-M relation is perfectly trivial and there is therefore no particular point in this G-language either, yet it might be worth while to discuss briefly in what respect the L-M relation differs from those symmetrical ones which we have called isomorphic or translational. As an instance of relations of the latter kind, take French for L_1, Spanish for L_2, English for the co-ordinating G. G contains the quoted expressions of the two foreign languages and, in addition, the " syntactical " predicates of the English grammarian, such as ' French sentence ' (' sentence$_1$ '), ' Spanish sentence ' (' sentence$_2$ '), etc. ; furthermore, in order to interrelate the two object languages, G contains the relational predicate ' synonymous ' which is, ideally speaking, symmetrical and has the quoted expressions of both foreign languages as its domain. The G which we use for the semantical correlation between L and S also contains two parallel series of predicates, ' particular ', ' predicate ', ' sentence ', etc., with the qualifying genitives ' of L ' and ' of S ' respectively and, furthermore, the *names* of such S-predicates as ' particular ', ' predicate ', ' sentence '. And this time the correlation will obtain, not between the two parallel series but between the L-series and another series, *about* S, which contains the names of the series *in* S. The relationship itself is established by means of a defined, asymmetrical predicate of G, which one might transcribe by ' p names q '. Following is the approximate transcription of a typical sentence of G : If q is a *sentence* of L and if p names q then ' p is a *sentence* ' is an (analytical) sentence of S. Of the two italicized occurrences of ' sentence ', which mark the members of the picture relation, the one stands within quotes, while the other does not.

As the terms have been used in an earlier section, such interrelating of L and S would not classify as a syntactical study, though of course as a formal one. But it will be remembered that as yet no definition has been offered of the term ' syntactical '; in using it I have relied upon its rather unambiguous denotation. The point is that in the last analysis no other than a denotational differentiation between pure syntax and pure semantics seems to be possible. The decisive feature which they have in common is that they are both formal. If the object language studied has

the structural features of a semantical system, in other words, if it deals with the schemata of the familiar semantical concepts of designation and truth, then the study is called semantical. If one evolves formal schemata bearing upon our notions of logical consequence and analyticity, without relating this idea to those of truth and designation, then the study is called syntactical. This is clearly a denotational differentiation, but it is not meant to deny that the distinction, such as it stands, is a very significant one. It will be remembered that in the account which has been given of pure syntax certain results have been called a partial formal vindication of our philosophical ideas about analyticality. It has also been said that the rest of this vindication lies in pure semantics. The idea is this. One and the same object language can be studied either syntactically or it can, by means of an S which contains its picture, *indirectly* become the object of an investigation in pure semantics. A synoptical and still formal consideration of these two independent studies can then be expected to yield, within the universe of symbols, results which justify our preconceived notions about the relationship between analyticality and truth. The most pregnant expression of these notions is, as is by now generally known, the so-called truth tables which have been invented by Peirce and Wittgenstein. All this can be done and has been done. Those studies to which Carnap now refers as the complete formalization of logic concern themselves with technical refinements of what, in a philosophical sense, pure semantics has already achieved. Whether they are of a nature to command general interest or should be left to the efficient efforts of the formal logicians I am not yet prepared to say.

V.

It has already been mentioned that the criticism to which this section will be devoted, does not affect the core, but only certain features of Carnap's presentation of pure semantics. These features, however, pervade most of his *Introduction to Semantics*. It seems to me that the best way to proceed under these circumstances is to select one or two characteristic points and to restrict my comments to them. And here it will be possible to be rather brief, since in the preceding sections the main emphasis has been laid upon just those features of pure semantics which provide the necessary background for the following remarks. The items which have been singled out for comment are, *first*, the dichotomy between sentences and propositions, and, *second*, the identity of object language and metalanguage.

In the statement :

(3)   ' Fido is a dog ' is true if and only if (Fido is a dog).

Carnap refers to the quoted expression as a sentence, while the bracketed expression is called a proposition.   In the terminology which has here been used the quoted expression, including its quotes, is a sentence$_M$ or, as we should rather write now, a sentence$_S$, while the bracketed expression, as well as (3) itself, are sentences of S, each of which, after they have been surrounded by quotes, yields an argument of the predicate ' sentence$_{MM}$ '. There is of course no harm in a terminology which refers to sentences of S as propositions as long as it is clearly understood that then ' proposition ' is a concept as strictly formal as ' sentence '. This, however, is not Carnap's meaning. In the terminological dictionary which one finds in section 37 of his book two usages of the term ' proposition ' are distinguished. One meaning is given as synonymous with ' sentence ', the other as " that which is expressed by a sentence ", a " state of affairs (Wittgenstein) ", and it is indicated that it is with the latter meaning that the term is used in the text.   In contradistinction to such views it is here maintained that *there is no room in pure semantics for propositions* thus understood, otherwise pure semantics would not be a formal discipline and hardly of any philosophical interest at all. The same criticism applies to the analogous terms ' individual ' and ' designata '. They, too, if they are to occur in pure semantics, do not refer to any extra-linguistic referents but are merely MM-predicates applying to those particulars and expressions of S which, by virtue of the designation relation, are designated by something.

Even if the point is granted, it still remains to be shown why I have singled out for criticism what might be considered as a minor terminological inaccuracy at the fringe of an impressive formal development. It has already been said that the main concern of this paper is epistemological. From the epistemological view-point, however, we are here not faced with a minor ambiguity but rather with a deviation from the positivistic epistemology in the direction of realism, or, at least, with the danger of such deviation. Certainly Carnap will be understood in this manner, if for no other reason than the use to which the term ' proposition ' has been consistently put during the last decades. That logical positivism, whenever it temporarily loses sight of Wittgenstein's fundamental thesis, is likely to tend towards realism is easily enough understood if one re-members that phase of this movement where physicalism and

behaviourism were taken for epistemological view-points rather than for what they actually are, namely, theses of the methodology of science which require an epistemological foundation. And there was, and still is, the reaction against the arbitrary limitations which Wittgenstein tried to impose on philosophical analysis.

There might be some technical reasons why in *Introduction to Semantics*, as it now stands, the term ' proposition ' cannot be simply replaced by ' sentence of M ' or ' designated sentence of M '. If there are such reasons, of which I am not sure, then I am further inclined to believe that they are to be found in that part of the work which deals with propositional calculi and in those cases where object language and metalanguage are said to be identical. If this is true then a rewording by means of a strictly formalistic terminology is advisable. Otherwise, to venture one more opinion for which I have no reliable grounds, it might happen that we shall run into paradoxes and that we shall be tempted to eliminate them by the kind of analysis which has been offered in *Principia Mathematica* for the semantical paradoxes, which analysis has since been recognized as mistaken. It will be seen from what has just been said why the so-called identity of object language and metalanguage has been chosen as the second item for comment.

Let us begin by listing the meanings which could be given to the assertion that two languages coincide or are identical. There is only one sense in which two discursive universes or uninterpreted languages can be said to be (partly) identical, namely, when the symbols and rules of the one are either a subset of the symbols and rules of the other or if the respective sets overlap. An overlap as to symbols alone does not seem worthy of any particular attention ; the best one can do in such case, particularly in any study which deals with both calculi, is to change the shapes of the symbols in one of them. On the other hand, two different discursive universes can show all degrees of isomorphism and conceivably this state of affairs could be referred to as an identity. It seems, however, that neither of these cases is meant when L and S are said to be identical. What is meant is rather that one and the same calculus " speaks about itself ". This is necessarily a statement about interpretation and the calculus in question can therefore not be S, since S is dealt with formally in pure semantics. What seems to be intended is indeed that L speaks about itself. Formally that means simply that certain particulars of S occur in both domains of the designation relation, that is, they both designate and are designated. There is, finally, still another alternative which could conceivably

represent what is meant by the identity of object language and metalanguage. One could assume the existence of certain isomorphic relationships (not the picture relation?) between L and S, and then see what becomes of the semantical theorems under this additional assumption. Materially this amounts to assuming that L and S have, or at least, could have in part the same referents.

VI.

In general the whole pseudorealistic terminology of designata, propositions, entities, and absolute concepts leads easily to a misappraisal of the epistemological significance of pure semantics. In the following points I have tried to summarize the various epistemological contributions of pure semantics which, I believe, mark a very significant step in the epistemological thought of Logical Positivism (Scientific Empiricism).

1. The unanalysed, traditional notion of truth must be split, not into two, but into three concepts, namely, (a) syntactical truth or analyticity as defined in pure syntax, (b) semantical truth, as defined in pure semantics, and (c) pragmatical truth. Some remarks about pragmatical truth will be found at the end of this paper ; but it can already be seen that one over-estimates the importance of the formal development if one asserts, as Carnap does, that the main feature which distinguishes seman-tical truth from the " old classical concept which goes back to Aristotle " (p. 29) is *precision*. The epistemological significance of pure semantics lies rather in the fact that, after it has helped us to realize the necessity of a threefold analysis of the traditional concept, it *formalizes* the second meaning of the pre-analytic notion. Here, as throughout this paper, to be formal means to be contained within the universe of symbols. It is the possibility of such formal analyses which substantiates the claim of the positivists that their anti-ontological epistemology is not necessarily rudimentary and is, possibly, complete. Formal *v.* nonformal, not syntactical *v.* semantical or pragmatical, is indeed the crucial dichotomy. Terminologically at least this has been anticipated by Carnap's earlier expression ' formal mode of speech ', at a time where the imperfect realization of the possibilities of semantics and pragmatics still led the positivists to force everything which was rightly felt to be non-factual into the Procrustes bed of syntax.

2. The complementary contribution of semantics which has been sketched in an earlier section completes the framework for the analysis of analyticity.

3. That the construction of pure semantics proceeds analytically without undefined descriptive predicates and without axioms represents, within the positivistic epistemology, the basic nominalistic thesis that semantical truth and designation are not natural relationships such as fatherhood, leftness, and betweenness.

4. That an adequate formalization of the truth concept (b) can be given in semantics, but not in syntax expresses the cognitive core of the so-called correspondence theory of truth. This becomes still clearer if one puts it negatively and somewhat paradoxically : The possibility of an adequate syntactical definition of semantical truth would express whatever sense there is in the idealistic coherence theory of truth. Since an unambiguous foundation of the correspondence theory is one of the essential tenets of ontological realism and has always been among the main concerns of its serious proponents, this result goes a long way towards the completion of a positive positivistic philosophy.

Pragmatics is defined as the study of the relationships between a language, its interpretation and its users. A pragmatical metalanguage P contains therefore the names of the expressions of the object language, the names of their referents and the semantical predicates ; briefly, it contains semantics in the same indirect sense in which semantics can be said to contain syntax. But P contains moreover another set of predicates which correspond to the verbs for the activities of the Self, such as ' knowing ', ' believing ', ' doubting ', ' verifying ', and so forth. Naturally, P is a very rich descriptive language. What would be the result of its formalization ? Any such formalisation, it seems safe to predict, will be an axiomatization in the proper sense of the term, that is, the verbs of consciousness would turn out to be descriptive predicates and many significant statements in which they occur, as for instance, ' A believes that p after he has verified p ' would turn out to be synthetic. I want to make it as clear as possible that I do not intend here to make any specific suggestions as to the structure of a correct and complete axiomatization, a geometry of consciousness. It will be noticed, for instance, that the cognitive verbs have not been described as primitive or undefined predicates, since the actual achievement of a behaviouristic psychology of the higher processes would probably amount to their definition in terms of descriptive relationships which lie " below " the level of consciousness.[1]

[1] The concept of L-range which Carnap has introduced in *Introduction to Semantics* is likely to prove very helpful in relating the basic terms of such an axiomatization to the verification level of consciousness.

Generally it can be said that a materially significant formalization of pragmatics *is* the axiomatics of an objective or, as one also says, behaviouristic psychology of the higher processes. Thus the matter is in the hands of the scientists and no immediate results, such as in the case of semantics, are to be expected. Yet the mere possession of the schema is clarifying, as I shall try to show by briefly discussing two points. The first concerns the relation between semantical truth and verification, the second touches, very hesitatingly, on the fundamental issue of the Self.

1. That the mental verbs and, in particular, verification cannot be formalized within semantics is as significant as the impossibility of formalizing designation and truth within syntax. Negatively this signifies that certain issues are basically scientific, not philosophical. Positively it holds the promise that in a full axiomatic development of pragmatics, we will be able to analyse the basic factual features of consciousness in a genuinely philosophical manner which goes far beyond even the scientific psychology of the future. Much relevant insight will also be derived from the tracing of those statements which will turn out to be analytic. That I never have your experiences, for instance, or some such transcription of the common sense core of idealism, might very well be a candidate for analyticity. Turning to more immediate issues one will remember that Russell, in the last chapters (XX-XXIII) of his recent *Inquiry into Meaning and Truth*, hesitates to accept the positivistic doctrine of verification as a theory of truth and tends towards realism because he feels that the alternative is not compatible with what he considers the law of excluded middle. The difficulty disappears as soon as one realizes that in those passages Russell speaks indiscriminately about semantical and pragmatical truth (verifiability) and, probably, also about analyticality.

2. The language that we all speak and which is the vehicle and, according to the positivistic bias, more than just the vehicle of our consciousness, is an *informal, universal* language, that is, it comprehends in a manner which leads to logical paradoxes and to philosophical puzzles, object language, syntax, semantics, and pragmatics, or rather, everything. Furthermore, as far as its informal pragmatic part is concerned, there is excellent reason to assume that in it the mental verbs, or at least some of them, are not only descriptive, but undefined or primitive descriptive relations. If this is true at all then it is entirely independent of present or future results of the science of psychology. But if this is true, as I believe it to be, then we are finally in a position to formulate the empiricist thesis about the

Self and at the same time give its full due to the introspectionistic evidence which has been used to combat it ever since it was first proposed by Hume. Hume would be justified by the fact that even in an exhaustive formalization of our empirical language nothing that corresponds to the Self would be found on the object language level, while on the pragmatic level the names of Self and of some of its activities might very well be among the primitive descriptive terms. The actual, articulate development of this schema would, to my mind, constitute the crowning achievement of Logical Positivism. And it could also be argued that a statement like this is not quite without historical justification, for the issue of the Self has never come to rest even within the empiricist tradition itself. To realize this it is only necessary to read the notes on memory, belief, and expectation, which John Stuart Mill added to his father's scientifically admirable *Analysis of the Phenomena of the Human Mind*. Reading them one feels almost as if one were allowed to listen to a dialogue between the scientific and the philosophical genius of modern empiricism.

17

IV.—DISCUSSIONS.

UNDECIDABLE STATEMENTS AND METALANGUAGE.

I

THE bearing which Goedel's undecidable statements have on the distinction between language and metalanguage was not made explicit in my *Problems of Logic*, and the lines to follow, while written with no claim to finality, are intended to clarify my tentative position.

I recognise, of course, the distinction between statements about things or events and statements about statements, but I do not think that this distinction implies, except perhaps as a methodological device, two languages to be contrasted with each other as the object-language, in which statements about things or events are collected, and the metalanguage, which consists of statements about the statements of the object-language. Statements of syntax, for example, although undeniably different in function from ordinary statements, need not form a separate language : the syntax of English can be given in English and so for other languages. Even the distinction between statements about things and statements about statements ceases to be basic as soon as we decide to identify statements with declarative sentences rather than with the propositions which the sentences express. For statements about, or descriptions of, printed sentences would seem to be ultimately reducible to descriptions of series of shapes of typographical ink on paper ; and such description is not different in kind from what a geographer says about configurations on a map or a mathematician about graphs.

II

To insist on opposing object-language to its metalanguage as two separate languages the mathematical logician must be convinced that the metalanguage contains, besides syntax, certain elements which, unlike syntactical statements, are not translatable into, or expressible in terms of, the object-language ; these recalcitrant elements, we are told, are the so-called semantic properties of statements, the properties of being true, false, analytic, and contradictory. Now this segregation of semantic properties comes about as follows. Goedel has shown how to construct within object-languages of a certain formalised kind a statement G which attributes a property P to itself. Goedel himself specifies or interprets the property P as " non-demonstrable ", and with this interpretation there is no difficulty ; but if we interpret P in terms of one of the semantic properties, G is transformed into an epistemological paradox or contradiction which is a form of the " Epimenides ". One way of avoiding the paradox is to prohibit the translation of metalinguistic semantic properties into the object-languages within which

G can be stated. But this prohibition strikes me as an *ad hoc* solution : the property of being demonstrable (or non-demonstrable) and the semantic properties are alike in being concerned with validation of statements, and there seems to be no reason for treating semantic properties differently except the fact that they alone generate epistemological paradoxes. Also the price for the segregation of semantic properties is too high for me : we are asked to denounce all inclusive languages, among them English, as inconsistent !

III

Let me try now a treatment of epistemological paradoxes without recourse to a metalanguage.[1] Consider the Epimenides' statement in the form :

(E) This statement is false.

Observe that the paradox takes place only if the designation " This statement " is used as a name of E. But in English such a use of the designation is incorrect ; a correct use requires a context within which the designation refers to *another* statement, usually immediately preceding the statement which contains the designation. The correct use may be illustrated by the following context :

The earth is flat. This statement is false. . . .

Suppose you try to state E in isolation from any such context. As soon as you utter the words " This statement . . .," I have a perfect right to interrupt : " What statement ? You use a designation of a statement, but you did not mention any. So the designation cannot be a name of a statement, at best it designates the statement-form ' The statement X ', where ' X ' is a variable." To this you reply : " Let me finish. I meant to say : This statement is false." But I cannot accept your explanation : " The property of being false must be attributed to a complete statement ; it is false or perhaps meaningless to attach ' false ' to a mere statement-form such as ' The statement X '."

This imaginary dialogue is intended to show why E, in isolation from a context of other statements, is bad English ; we are forced to construe E as a falsehood or absurdity, to the effect that a statement-form is false and is, therefore, a complete statement.

Again let us use " p " to name the expression " p is false ". According to this convention we can substitute for the variable in the expression " p is false " the expression itself, and if this expression were a complete statement, there would be a paradox. But observe that no matter how many substitutions you perform, you cannot get rid of the variable " p ". Hence, contrary to the requirement that " false " should be attached to a complete statement, you are forced, at any step of your performance, to attach " false " to a statement-form. The result is not a paradox, but a

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[1] A recourse to Russell's " vicious-circle principle " is of no avail if Goedel's undecidable statement (which is about itself) is to be accepted as meaningful.

falsehood or absurdity, to the effect that a statement-form is false and therefore is a complete statement.

In a letter of comment upon the foregoing Dr. Quine wrote to me as follows :

But (the above treatment of epistemological paradoxes) does not help when we revise the Epimenides in some such form as this: " The statement written on the blackboard in Room A, Emerson Hall, Jan. 1, 1939, is false "—supposing the contents of this very quotation to have been the only statement on that blackboard during the holiday in question.

Let Dr. Quine's revised statement of the Epimenides be called Q. Also, for the sake of clarity, let us conform to Peirce's distinction between statement-token and statement-type. Then, to follow Dr. Quine, we must suppose that a statement-token of Q was written on the blackboard in Room A, Emerson Hall on Jan. 1, 1939. But observe that another token of the same statement Q appears on this page, within the context of my quotation from Dr. Quine's letter. Let me call the first token T1 and the second T2. Now I submit that T2 is not a paradox, and since T1 is just another vehicle for the same statement, T1 cannot be a paradox either. The point is that both T1 and T2 refer to some other statement than Q. The fact that on Jan. 1, 1939, there were no other statements on the blackboard in Room A, Emerson Hall, does not transform Q into a statement which is about itself. It would seem that Dr. Quine's paradox is given on the premise that a truth-value can be ascribed to a token-statement in disregard of the corresponding type-statement and of other tokens of the same type. I reject this premise ; to ascribe a truth-value to a token as if it were a type is no better than—in fact it is a fallacy of the same kind—to confuse a statement-form with a complete statement.

IV

My treatment of the epistemological paradox is not complete unless I can extend it to Goedel's undecidable statement G when the property P which G attributes to itself is interpreted as " false ". For one must accept the conclusion that G is a statement about itself even in isolation from a context of other statements. There is a temptation to argue that G is constructed within a formalised object-language and must not be translated back into an inclusive language (with a non-explicit syntax and logic) such as English. But in a recent article in MIND (Vol. 51, N.S., No. 203), on " Goedelian sentences : a non-numerical approach ", Dr. Findlay has succeeded in constructing an equivalent to Goedel's undecidable statement G within the medium of plain English. Let me turn then to an examination of Dr. Findlay's construction.

Dr. Findlay's construction of the undecidable statement proceeds in seven steps, but I shall be directly concerned with the fourth and seventh steps, to be called, respectively, Findlay's IV and VII. Findlay's IV is as follows :

IV. We now construct the following incomplete description of a statement : it is ' the statement which is arrived at by substituting for the variable in the statement-form Y the name of the statement-form in question '. This description is incomplete because it contains the variable ' Y ' : it would become a complete description if we replaced ' Y ' by the name or description of some actual statement-form. Thus if we replaced ' Y ' by " 'X is the first sentence in Euclid'", our description would run "The statement which is arrived at by substituting for the variable in the statement-form, 'X is the first sentence in Euclid', the name of the statement-form in question ", and this description would describe the statement " ' X is the first sentence in Euclid ' is the first sentence in Euclid."

The incomplete description in single quotation marks introduced at the beginning of this quotation, to be called I, is the basis upon which the undecidable statement, to which I shall refer as F-G, is constructed in Findlay's VII. By substituting " false " for " we cannot prove " in F-G we obtain the F-E version of the Epimenides which runs thus :

The statement is false which is arrived at by substituting for the variable in the statement-form, ' The statement is false which is arrived at by substituting for the variable in the statement-form Y the name of the statement-form in question ' the name of the statement-form in question.

As a preliminary to an examination of Findlay's construction I wish to introduce a distinction between names and pseudo-names. The expression " The statement p is false " may illustrate the distinction. The variable " p " is used as a name if it designates, by convention, some actual statement, such as " The earth is flat ", which is independent of the expression in question. The same variable is used as a pseudo-name when it designates the expression, " The statement p is false ", or some other expression whose meaning must be derived from the meaning of this one. As explained in the preceding section, the use of the pseudo-name " p " leads to the Epimenides in one of its traditional versions. Now I shall extend my treatment of the " Epimenides " to F-E by arguing that the latter also involves a pseudo-name, and therefore must be resolved in the same way as its traditional prototype.

Consider, first, the description I in Findlay's IV. The variable " Y " in I can be replaced by a name of some statement-form which is independent of I, as illustrated by Findlay's own choice of the name " ' X is the first sentence in Euclid.' " But observe what happens with such a choice. The statement in double quotes at the end of Findlay's IV contains an actual statement-form in single quotes, which means that it refers to, or is about, a statement-form and not a complete statement. Thus the statement in double quotes asserts that a certain statement-form is a complete statement, viz., the first sentence in Euclid, and an assertion of this kind is false or absurd. A similar result would follow the replacement of the variable " Y " by any other real name of an independent statement-form provided Findlay's I is to yield a comment on a complete

sentence.   If Dr. Findlay avoids this result in his F-G, the reason
is that the name of the statement-form within F-G depends on the
meaning of I, *i.e.*, involves the variable " Y " of the latter, and
therefore is a pseudo-name.   Hence the name given within F-E
in single quotes is also a pseudo-name, and is no better than, say,
a certain meaningless label " L ".   This is to say that F-E can
be exposed as if it were the following false or absurd pronouncement :

The statement is false which is arrived at by substituting for the
statement-form ' L ' the name of the statement-form in question.

While these considerations would seem to dispose of F-E, they
need not be construed as an objection against Goedel's G or F-G.
Without going into detail let me merely point out that G attributes
to itself the predicate " non-demonstrable " and this predicate,
unlike " false ", depends exclusively upon the form of statements,
and therefore may be correctly ascribed to expressions of suitable
form even if they involve pseudo-names in their construction.

If this suggestion proves to be a mistake, my position would be
that Goedel's undecidable statement is a legitimate construction
only within the context of his formalised language.   But I must
admit that this position has its unattractive side : it furthers the
estrangement between ordinary and technical languages.   Also
Goedel's own opinion is that Findlay has succeeded in translating
the undecidable formula into plain English.

V

I do not know whether my treatment of F-E will be found
satisfactory.   If not, some other method may be tried.   For
example, I still think that the principle, which I used in *The
Problems of Logic*, to the effect that any statement claims truth
and therefore cannot attribute falsehood to itself without self-
contradiction, is effective in resolving the " Epimenides " in its
traditional as well as in the Goedelian form.   And this principle
does not invalidate Goedel's G either, since the latter does not
attribute falsehood to itself.   But even if all proposed principles
for the resolution of epistemological paradoxes were in turn dis-
credited, there remains the expedient of introducing within an
inclusive language special conventions to disallow such formations
as the " Epimenides ".   Of course these conventions would be
prohibitions *ad hoc* ; even so they should be preferable to the
awkward idea of an infinite hierarchy of languages and to the
preposterous notion that such inclusive languages as English are
intrinsically self-contradictory.   Let me end with a note of warning
to logicians who insist on the notion that inclusive languages are
self-contradictory.   A self-contradictory set of statements implies
any statement, true and false alike.   Therefore in arriving at the
notion of a self-contradictory inclusive language within the medium
of this very language, as one perforce must, one may be arriving
at a false conclusion implied by a contradictory set of premises.

<div align="right">A. USHENKO.</div>

THE "PARADOX OF ANALYSIS".

PROFESSOR MOORE has recently provided us with a welcome discussion of the sense in which he has in the past used (and "intended to use ") the word analysis.[1] A feature of Moore's explanation which ought to remove some confusion from the minds of his interpreters is the explicit stress he now lays upon the non-verbal character of the objects of analysis : " . . . I never intended to use the word [' analysis '] in such a way that the *analysandum* would be a *verbal expression*. When I have talked of analysing anything, *what* I have talked of analysing has always been an idea or concept or proposition, and *not* a verbal expression. . . ." [2]

In what follows I shall adopt Moore's usage and speak of *concepts* as the meaning of certain expressions but the argument will be independent of any positive view of the nature of the entities designated by the expressions in question. Wherever the term "concepts " occurs the reader might substitute the less definite term " designata " [3] without detriment to the discussion.

Since analysis is to be regarded as concerned with concepts, it becomes plausible to interpret any analysis, say that of " brother " into " male sibling " (Moore's example), as expressed by an assertion of *identity* between concepts. If we follow Moore, however, in expressing the analysis by some such statement as :

(1) The concept " being a brother " is identical with the concept " being a male sibling " [4]

It seems possible to derive a troublesome paradox : " The paradox arises from the fact that, *if* this statement [(1)] is true, then it seems as if it must be the case that you would be making exactly the same statement if you said : ' To be a brother is the same thing as to be a brother.' But it is obvious that these two statements are not the same ; and obvious also that nobody would say that by asserting

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[1] *The Philosophy of G. E. Moore*, Ed. by P. A. Schilpp (1942), pp. 660-667.

[2] *Op. cit.*, p. 661. The statement is part of a reply to a critic in the same volume who suggested that Moore intended to analyse verbal expressions. *Cf.* C. H. Langford, " The Notion of Analysis in Moore's Philosophy," *op. cit.*, pp. 321-342. A similar mistake has frequently been made, *e.g.*, by A. E. Duncan-Jones : " An *expression*, *y*, is an analysis of an *expression*, *x*, etc." (*Aris, Soc. Supp.*, Vol. 16, p. 143).

[3] Or, possibly, " connotata ". But clearly not " denotata ".

[4] Moore provides three more ways of expressing the same analysis. The form he uses in deriving the paradox is " To be a brother is the same thing as to be a male sibling." I presume the variation in expression does not affect his argument. *Cf. op. cit.*, pp. 664-665 for a discussion of the alternative formulations.

' To be a brother is to be a brother ' you were giving an analysis of the concept ' brother.' " [1]

It is the purpose of this note to show how this paradox, to which Moore is unable to find a solution, may be circumvented.

Since we shall constantly be referring to the concepts, being a brother, being a male, being a sibling and being a male sibling, let us designate them respectively by the symbols ' b ', ' m ', ' s ' and ' ms '.

Moore is saying that if (1) is true " it seems as if it must be the case that " (1) is " exactly the same statement " as :

$$(2) \qquad\qquad b = b.$$

While no reason is given for this assertion it may be supposed that it is based upon the assumption that (1) is properly symbolized as :

$$(3) \qquad\qquad b = ms$$

and the supposed consequence that both (2) and (3) are expressions of the same tautologous identity.   It is in attempting to avoid this unwelcome conclusion that Moore is led to inquire, without finding a satisfactory answer, whether (1) may not really be " a statement *both* about the *concept* brother and also about the two verbal expressions used." [2]

Before following Moore's search for a semi-verbal interpretation of analysis let us first examine whether the expression of (1) as the identity (3) is not gravely misleading.

In order to test the argument introducing the paradox we may for a while consider the numerical equation :

$$(4) \qquad\qquad 21 = 3 \times 7.$$

Suppose someone were to argue in this fashion : " A paradox arises from the fact that if this statement, $21 = 3 \times 7$, is true, then it seems as if it must be the case that you would be making exactly the same statement if you said : ' To be 21 is the same thing as to be 21 ', *i.e.* :

$$(5) \qquad\qquad 21 = 21.$$

But it is obvious that these two statements are *not* the same ; and obvious also that nobody would say that by asserting (5) you were giving the same information as by asserting (4)."

In this argument we have conjured up, the source of the fallacy is sufficiently obvious.   The symbol ' 3×7 ' is not merely an alternative *name* for the integer 21 ; and that integer is not the sole referent of (4).   Indeed (4) is rather a statement to the effect

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[1] *Op. cit.*, p. 665.   This is an instance of what Langford calls the " so-called paradox of analysis " (p. 323).   It is, perhaps, the recognition of this paradox which leads Langford and others to suppose that analysis ought to be concerned with verbal expressions.

[2] *Op. cit.*, p. 665.

that the *three* numbers, 21, 3 and 7, stand in that relation which holds between three integers whenever the first is the product of the remaining two. If this relation be designated by ' A ', a less misleading way of symbolizing the proposition expressed by (4) is, accordingly :

(6)   A(21, 3, 7)

where the preferred symbolism reminds us that a three-termed relation is involved.

If this is correct, it is obvious that the identity (5) is not to be regarded as " the same statement " as (4). For no plausible interpretation of " being the same statement " would seem to permit the assimilation of a mere identity with a proposition in which a non-identical relation is a component.

The application to our original example is immediate. Just as triples of numbers may stand in a certain relation, exemplified by (21, 3, 7), (18, 2, 9), (15, 3, 5), etc., and previously designated by ' A '; so we may recognise triples of ' concepts ' as standing in a certain relation exemplified by (being a brother, being a male, being a sibling), (being a mare, being a female, being a horse), (being a square, being equilateral, being a rectangle), etc.

It is a little awkward to refer to the latter relation explicitly, in the absence of any familiar correlate for the term ' product ' used in the numerical illustration. It is allowable, perhaps, to speak of the concept male sibling as the *conjunct* of the concepts male and sibling. Then we can say that the relation in question, which may be designated by ' B ', holds between three concepts whenever the first is the conjunct of the remaining two. And a less misleading form for the proposition expressed by (3) is, accordingly :

(7)   B(b, m, s).

For the same reason as before, there is, in the light of this discussion, no justification for regarding (7) as expressing the same proposition as that presented by (2), and the argument which engendered the paradox collapses.

I conclude therefore that even if we interpret Moore's example of an analysis as being *only about* ' concepts ' there is no good reason for regarding the analysis as issuing in an *identity* between concepts ; and no further measures are needed to take care of the " paradox of analysis ".

It may, nevertheless, be worth while to examine whether an analysis, such as that expressed in (1), may not prove capable of interpretation, after all, as " a statement *both* about the *concept* brother and also about the two expressions used." For if this should prove successful it would provide us with an alternative mode of analysing analysis acceptable to those who have independent grounds for believing that analyses make some reference to expressions.

There are, moreover, certain propositions which appear to be identities, and cannot be interpreted in the manner adopted above. Consider for instance the statement

(8)   To have inertia is the same thing as to have mass

which might be asserted by somebody who had progressed sufficiently far with the study of mechanics to be able to recognise the identity of the somewhat complex concepts mentioned.   In such a case as this, the solution proposed above would clearly break down; for the concept of having mass, unlike the concept of being a male sibling, is not the result of an operation upon *more than one concept ;* and we cannot interpret (8), it seems, as involving a relation between more than two concepts.

One may not wish to regard (8) as the expression of an ' analysis '; but (8) may very well convey information and is, on that account, by no means " the same proposition " as :

(9)   To have inertia is the same thing as to have inertia.

We must therefore search for some interpretation of the proposition expressed by (8) which will not require us to regard it as a mere identity between concepts.

The most obvious suggestion, *viz.*, to regard (8) as equivalent to an equation of meaning between the expressions " to have inertia " and " to have mass " will not do, for the reason pointed out by Moore.   An analysis (in the sense intended by him) could never take the form of a statement to the effect that two verbal expressions merely had the same, but unspecified, meaning :

" A man might point out to me two expressions in a language of which I was completely ignorant and tell me that they had the same meaning, without telling me *what* they meant. . . . But if this were all he was doing, he would not have told me anything at all about any *concept* or *idea*, which either of the expressions expressed ; and would therefore certainly *not* have been giving me an analysis of any *concept.* . . ." [1]

Now (8) presumably *is* about a " concept or idea " and not solely about expressions.

Let us, however, interpret the statement (8) as having exactly the same meaning as :

(10) The concept having inertia is designated by the expression ' having mass ' and the concept having mass is designated by the expression ' having inertia '.

If we use ' D ' for the relation *being designated by* which holds between a concept and its name ; ' I ', ' M ', as symbols for the concepts *having inertia* and *having mass ;* and, finally, ' i ', ' m ', for the

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[1] *Op. cit.*, p. 662.

expressions, ' having inertia ' and ' having mass ', we can write (10) in the shortened form

(11) $$D(I, m) \& D(M, i).$$

This assertion seems to mention two concepts, I and M, and two expressions, i and m, but we shall soon see that I and M are identical. What we have in fact, in (10) or (11), is an assertion which indicates that the expressions i and m are synonymous *by mentioning the concept which both designate*. It is, therefore, an assertion about a *single* concept (*viz.*, that of having mass or having inertia) and two expressions (*viz.*, ' having mass ' and ' having inertia ').

By making certain plausible assumptions concerning the conventions of symbolism of the language in which i and m occur,[1] we can assert the following three propositions :

(12) $$(x)(y)(z)\{(D(x, y) \& D(z, y)) \supset x = z\}$$

(i.e. D is a one-many relation)

(13) $$D(I, i)$$

(i.e. the concept, having inertia, is designated by ' having inertia ')

(14) $$D(M, m)$$

(i.e. the concept, having mass, is designated by ' having mass ').

From the conjunction of (11), (12), (13) and (14) we can in turn deduce

(15) $$I = M$$

and

(16) $$(x)(D(x, m) \equiv D(x, i)).$$

Of these the first asserts the identity of the concepts, while the second asserts the synonymity of the expressions. Now we have seen that if (11) were the same as either (15) or (16) it would need to be rejected. But clearly this is not so : for (15) is a statement about concepts only and (16) a statement about expressions only, while (11) is about both.

The application to Moore's example, (1) above, is obvious.

MAX BLACK.

[1] A discussion of these assumptions would involve us too far with some debatable questions of semantics.

V. CRITICAL NOTICE.

Mind: Perception and Thought in their Constructive Aspects. By
PAUL SCHILDER. Columbia University Press; London, H.
Milford, 1942. Pp. xii + 432. 33s. 6d.
Goals and Desires of Man. By PAUL SCHILDER. Columbia Uni-
versity Press; London, H. Milford, 1942. Pp. xii + 305.
26s. 6d.

THE classification of philosophers is a somewhat frivolous occupation,
but there is one scale apposite in this connection, and that is the
scale which ranges between the relatively static conception of man's
place in, and knowledge of, the world and the relatively dynamic
and biological conception. At one end of the scale you have the
view of common sense : the world is there, static and firm in out-
line, sitting in space and enduring in time, with its objects at rest
or in motion. On to this world the baby opens its eyes, seeing
what there is to see and learning suitable adjustments to the objects
and persons about it. Philosophers quarrel about the ultimate
status of the objects—are they partially as they appear, and par-
tially not ? are they a cinematographic performance provided by
God ? or are they the way we see things of unknown nature as we
peer through our categorical spectacles ? Anyway the world is
there, and we are perceiving it and thinking about it. At the other
end of the scale human living experience takes pride of place.
Action dominates the scene, and what we think and what we see are
dictated by our living needs—the world is the result of a creative act.
  Paul Schilder, the writer of these two books, is to be placed towards
the activistic, biological end of the scale. For such thinkers the
resistance to Life presents a problem. Matter indubitably seems
to be there, somehow or other, but obviously, if you lay your full
stress on creative activity, your view about what Life is up against
is bound to be imprecise.
  Schilder is not a philosopher and is not very much bothered by
such problems, but they are implicit in his contribution to psychology.
He does certainly give us an indication of his views. " Human
experience " he says in *Mind : Perception and Thought,* " consists
in living at once in these three spheres " (self, world and body)
" which form an inseparable unit. We may call the fact that
experiences are experiences in the outside world, in the body, and
in the self, an *a priori* insight " (*M : P.T.* 390). But " I do not
think that sensations are projected into the outside world and so
become perceptions. The division between outside world and
body is always present " (*ib.* 372). And " In speaking here about
sequences in reality, objects, and configurations, I imply without

hesitation that such sequences, objects, and configurations which have been established through action by the process of trial and error are indeed part of this world " (*ib.* 325).

In spite of the last quotation, which seems to imply a world for which we grope and which would place him towards the common-sense end of the scale, it will be seen that the whole accent of the first of these books is in the other direction. Like most of us he wants to have it both ways.

Schilder, who died with tragic suddenness in 1940, was Research Professor of Psychiatry at New York University Medical School. He was a prolific writer, and published a number of books in Germany before he went to America in 1930, and further volumes have appeared since then.

At the time of his death he was in process of completing a four volume series of which two had already appeared, and these two works are the two final volumes. They all deal with the same general theme : the creative element in our experience of and thought about ourselves and the world in which we live.

The material on which he draws is mainly pathological and is of enormous interest and importance. Here is a man, interested in the general problems of psychology, who had immensely wide experience of just that field which the philosopher and the theoretical psychologist know so little about. He seeks to apply clinical findings to the elucidation of old-fashioned laboratory questions : space perception, colour vision, configuration and the like. This is the subject-matter of *Mind : Perception and Thought*, by far the more interesting of the two.

One supreme difficulty, however, must be mentioned. Whether the ' editing ', referred to in the prefatory remarks is responsible, or whether Schilder himself was too lively and creative to be bothered with making an orderly statement of his views, when he had so much to say, the fact is that both volumes tax the reader's endurance so much that it is almost impossible to be sure that one has grasped the essential points. Sometimes one gets the impression of a collection of lecture-notes with a bit of an article popped in here and there with the result that a corresponding confusion reigns in one's mind, as one jumps from subject to subject.

The account which follows is therefore put forward with some diffidence.

The first question is : what is primitive experience like ? Do we start off with the stationary objects with their hard straight lines, their orderly arrangements, their angles and their position in space ? If not, what *do* we start with ?

Schilder uses four sources of evidence : (1) The drawings of children, and such evidence as there is of their capacity to recognize objects ; (2) experiences reported from pathological material, (*a*) associated with brain lesion, (*b*) in connection with the influence of drugs, (*c*) associated with pathogenic diseases, and (*d*) found

in cases of recovery from total blindness ; (3) the phenomena of synæsthesia ; and (4) tachistoscopic presentation.

Since we can have no knowledge by acquaintance of primitive experience in the ordinary way, we have to approach the matter indirectly. These four sources depend, therefore, for their value on certain assumptions : (1) that the child sees as it draws. There is a certain plausibility here, because in some experiments performed by L. Bender on the copying of simple ' Wertheimer *Gestalt* figures ' there seems to be a progressive approximation to accuracy from spirals and squiggles to fair representation, as we pass from one age group to another. (2) That all pathological conditions are regressive and allow the manifestation of out-grown processes. " In disintegrating cerebral lesions the gestalten tend to revert to more primitive levels and as the brain recovers from its injury they tend to follow the laws of developmental maturation in returning to the higher integrating responses " (*M : P.T.* 150). There is, of course, evidence to support this, and the doctrine that as we develop we pile control upon control so that disturbance of the ' higher centres ' allows the ' lower centres ' to operate freely, is well known. How far it can be generalized it is difficult to decide. Schilder puts in as evidence the results of experimental research into the influence of mescal on perception as revealing primitive modes of experience. At first reading one is bound to exclaim that the drug is directly responsible for the phenomena, but perhaps it acts precisely through depressing the more complicated developments and thereby releasing the less complicated substratum.

The next assumptions are : (3) that synæsthesia is indicative of an undifferentiated condition of general sensibility, and (4) that tachistoscopic presentation is primitive in its characteristics.

Furthermore, there is, of course, added plausibility given to a theory if there is a convergence of evidence from various sources, no one of which, taken by itself, lends very much weight.

What then is the evidence ? From pathological cases of various kinds (brain-lesions, psychogenic, and drugs) we find a tendency in vision, audition, touch, etc., for the patient to experience disorientation in space, a general motility and fluidity in the objects around him, and a disorientation in time. Straight lines and angles give way to curves and spirals ; static objects move and dart back to their place, changing in size and shape ; forms are broken up into their constituents ; sounds are ill-localized and objects tend to be doubled.

The blind, when they regain their vision and can be induced to pay any attention to it, are similarly placed as regards spatial disorientation and the clear-cutness of object-vision.

Children at early ages draw in curves and spirals ; " Fixed points are difficult ", writes Dr. Bender, " and straight lines are not accomplished as the shortest distance between two fixed points, but as an expression of radiating tendencies " (*ib.* 146). And " W.

Stern found that children may perceive objects rotated 90 to 180 degrees from their position. Children more readily than adults recognize objects which are turned round, upside down, or from left to right " (*ib.* 50).

In certain tachistoscopic presentations the objects appear to change their shape.

In fact the conclusion is drawn that : " In primitive experience the size of objects is variable ; the location of the experiences is uncertain. Variability in size, number, direction, space, distance and rhythmicity is probably the most important characteristic in the primitive visual field " (*ib.* 373).

What, however, stands out for Schilder in all the senses as characteristically primitive is *movement*. " We at once reach the conclusion ", he says, " that as in vision the simple touch has a quality of movement in itself and that the primitive perception is not static but in motion " (*ib.* 63).

It is motility that Schilder sees as the connecting link between the modes of sensibility. ' Brightness '—a famous claimant as the basis of synæsthesia—is admitted as a characteristic of sound and sight, but motility is, according to Schilder, the basic feature of all primitive perceptual experience.

But there is no still watching self that elaborates the whirling moving elements into a coherent picture. Corresponding to the moving, shifting, disintegrating, unoriented data from all the perceptual fields is the rhythmically palpitating, perpetually moving, exploring organism.

And so we get some such picture as this. The body, a whole reacting and exploring apparatus, has three tasks : to keep itself stable and upright, to nourish itself, and to find out about the world, its own body and other people. There is a constant inter-action between all these functions. Therefore the primary con-trolling apparatus is the vestibular organ, disturbances in which affect the sensibility of *all* modes. The constant response to gravity is the background on which perpetual exploration, vegetative dis-turbance and action take place. Gradually, by grasping, seeing, touching, smelling, tasting (for " all senses finally lead to objects which appeal to all senses " (*ib.* 175), by trial and error we pin objects down in their place, fixing their colour to them, and now at last we can act adequately towards them. We do not act in a world we know, we rather know the world in which we act, and all action involves the vegetative and emotional as well as the cognitive functions.

But for adequate action we have to register past experience and use it for future purposes. So, too, memory and thought are fluid, groping streams of images and signals, which have by a process of trial and error to be brought down to stable corre-spondence with what really happened, and accurate registration of what the sequences of the world really are. A collapse of the

' reality-testing ' thought development is to be seen in the schizo-
phrenic, who takes the sign for the referent and lives in a world of
images and signals which he does not direct towards reality.

When we cease action we fall asleep, so that consciousness " is
in this sense readiness for action " (*ib*. 342). " By consciousness
we mean, then, that constructive psychic work which goes on con-
tinually, which tries to bring the data in the outer world into a
context, and which helps us to a deeper insight into the structure
of our psychic experiences " (*ib*. 341).

And the book draws to a close with two chapters on Geometry
and Physics and ' Antinomies ', which will appeal to the Logical
Positivists. Geometry and Physics are, as would be expected,
abstractions from certain real situations raised to an eminence of
unmerited dignity ; they only have validity in so far as they assist
our actions. Logic, with its Principle of Contradiction, is a crystal-
isation of the fluid opposites which experience brings. Concepts
are the focussing points of all kinds of ' incompatible ' attitudes,
which then get analysed into abstractions and loose their irridescent
reality in the cold and irrelevant light of pure reason. ' Before
and after ', ' love and hate ', ' determinism and indeterminism ',
' the finite and the infinite ' are removed from their contexts, in
which they are, as it were, transcended, and taken seriously as
alternatives which must either be so or not so. " No situation exists
in which human beings are devoid of the possibility of acting, hence
space must be unlimited. On the other hand, since individuals
sometimes are paralysed by conflicting possibilities and do not see
' a way out ', there must also be the idea of a limited space " (*ib*.
356). " Thinking in polarities is one of the fallacies of merely verbal
thinking. There are no opposites in nature " (*ib*. 360).

Finally, there is the social element. In our knowledge of our
own bodies we use the knowledge of the bodies of other people,
and *vice versa*, and furthermore the knowledge of our bodies and
the knowledge of things is reciprocal, so that things are social, and
even our knowledge of ourselves is social too. " We observe our-
selves in the same way that we have been observed by others,
especially by our parents. Self-observation and self-consciousness
are therefore acts of a social nature and are the expression of the
fact that human beings are basically social " (*ib*. 335).

This account of the bones of the argument necessarily leaves out
much of the exuberant flesh with which they are clothed. The
detailed account of disturbances of the vestibular organ is ad-
mirable, and, as has been indicated, occupies a key position. The
chapter on Memory contains an account of a serial memory experi-
ment—rather like those described in Bartlett's *Remembering*—in
which Korsakoff patients took part. This is of interest because
in this disease the normal capacity for remembering is gravely
impaired, but some ' schematic ' trace is left by stories which have
been heard, and the recall, while fantastically incorrect, bears some

echo of the original. We have, therefore, an interesting exercise in creative memory and symbolism. The technique is, of course, discussed in connection with the investigations of Botlheim and Hartmann. Then, again, there is a good chapter on the development of language from unorganized utterance to accurate verbalization.

One note, however, is struck which leads us to the second of the two books under discussion. "We need", says Schilder, on page 176 of *Mind : Perception and Thought,* "We need the outward world which we are continually perceiving. It is in our organization to have before us a perpetual outside world."

Now the main theme of *Goals and Desires of Man* is that construction and preservation are the essential basis of life. "The psychological necessity to wish for independent objects is obvious", he says on page 201. And, again, on page 268 : "Concerning human beings we need love objects, and we want these love objects to retain their physical and mental integrity".

From these quotations it will be seen that the second book is concerned with human relations, and indeed it is a contribution to the general theory of psycho-analysis.

The insistence on a constructive principle naturally leads to a denial of the Freudian destructive principle—the 'death wish', and this involves an interesting investigation into the theories about, and attitude towards, death as observed in children and adults. But besides this direct attack, in which he shows that there is no evidence for a 'death wish' in the ordinary sense of the term (Freudians may ask whether he is not arguing rather beside the point), he also has to consider the phenomena of aggression. Here he makes an important point. Aggression is not a single category of behaviour—there is aggression and aggression. Sometimes rough behaviour, which may be destructive in fact, is exploratory and even constructive in principle.

This deprecation of the use of abstract words, apart from their context, is valuable, and Schilder makes good use of it when he deals with homosexuality, and the distinction between masculinity and femininity. He also points out how the culture pattern determines the meaning and opportunities for various kinds of behaviour.

Again, the main theme is confused by frequent digressions into side-issues, and there is not space to follow him further.

Perhaps enough has been said to indicate the main theses. Both books contain a wealth of material so luxuriant that it cannot be summarized, but so rare that it well repays investigation. If advice may be offered, it is that the reader should in each case read the last chapter called 'Conclusions' first ; it is then hoped that he will the more fruitfully poke about among the chapters which have preceded it.

W. J. H. SPROTT.

18

VI.—NEW BOOKS.

Freedom Forgotten and Remembered. By H. KUHN. University of North
Carolina Press and Humphrey Milford. Pp. 267. $2.50 and 15s. 6d.

THIS is a hard book for the English reader to assess. The syntax, as
we should expect, is irreproachable, the usage of words, though sometimes
unusual and always highly abstract, is intelligible, but what may be
called the intellectual idiom, in its blend of argument and rhetoric, is
baffling. Perhaps it has an easier appeal to an American audience.

Where the sailing is plain, the conditions of the Nazis' rise and the
character of their rule are briefly and vividly described : the resentment
at the peace terms, the desperate misery of inflation, the end of unem-
ployment by rearmament, the concessions by the Great Powers to Hitler
which had been refused to Weimar ; then the *Gleichschaltung,* the con-
centration-camp, the pogroms. All these are brilliantly sketched in the
second and third parts : 'The Action of Despair' and 'The Logic of
Passion'. The beginning of the fourth part, 'The Message of Hope',
also gives an outlook, lucid enough if necessarily vague, on a future
worth fighting for.

But all this, in the author's view, is subsidiary to his main purpose,
which is 'metaphysical'. It was not, after all, the material situation
of Germany which gave the Nazis power, or reconciled our statesmen to
it. The ease of their victory and the horrors of their rule were but symp-
toms of a mental sickness, the 'Forgetting of Freedom', endemic and
fatal in totalitarian states, curable and sporadic in the democracies,
the loss of a theological and indeed Christian metaphysic. And here the
book makes harder weather.

It is not clear what the author means by freedom. Political freedom
is said to be a result of the belief in it (p. 28). Is it then freedom of the
will, freedom to choose between alternative actions ? Apparently not,
for we are told (p. 13) that there are degrees of it depending on " the
ability of choosing and the adequacy of the guiding knowledge ". And
we constantly meet the strange phrase, " freedom of conscience ". This
cannot be social or political freedom, for nobody can coerce consciences
as he may words or actions. Nor can it be freedom of choice, for nobody
can choose whether to recognise a duty any more than whether to be
convinced by a proof. Again we are told of the 'fight for freedom' and
that 'freedom is an arduous task to be achieved'. But if we are not
already free, either we must or cannot achieve it. I guess that by the
' forgetting of freedom ' is meant the forgetting that we really have duties.

This forgetting, the author thinks, can either be, like that of the total-
itarians, a wilful denial, on the one hand by the historical-relativists
like Dilthey (and Hegel ?), and on the other by the irrationalist will-
worshippers like Kierkegaard and Nietzsche ; or it can be, like that of
the democracies, the slackness and complacency of 'positivists' (logical
positivists or Comtists ?) and pragmatists.

What is meant by the 'Logic of Passion' is still obscure. 'The logic
of the Nazi movement is the logic of passion' (p. 101). Yet "Reason,
in a supreme effort, discovers, on the outskirts of its limited domain,
God the creator of free beings. But venturing forth to this boldest

assertion, Reason is impelled by Passion, *i.e.*, by the urge of an absolute aspiration and a radical fear " (p. 113).

The only ' Recollection of Freedom ' Dr. Kuhn envisages is a return to Christian theology with its historical and eschatological dogmas. Between that and sheer amoralism, between Hobbes and Paley, he seems to admit no philosophy. Yet at times he is prepared to equate God with reason (p. 251), with truth (p. 255) and even with Enlightenment (p. 257), and claims not only Plato and Aristotle but Bertrand Russell as allies. Surely the former secretary of the Kant Society must be as familiar as anybody with a philosophy teaching that the reality of duty is the most certain fact we know and its performance the greatest good ; that duty implies the treatment of every man as an end, and presupposes freedom ; a philosophy which allows indeed that a ' faith ' in God and immortality *may* be founded on such knowledge, yet insists that ' rational theology ' is impossible.

I think most of what Dr. Kuhn is contending for has been more shortly put by the communist Wilhelm Koenen in ' Germany's Road to Democracy '. " If you ask me what first to teach the German Youth when the shock of defeated Hitlerism has been administered, I will say : ' Give them some of the Ten Commandments, which have been at the basis of Judaism, Christianity and every modern civilisation. Hitler has put back the clock to the point where the elementary and fundamental principles were conceived and the groundwork of ethical humanism laid by the exhortations not to kill, and not to steal and to honour each our father and mother." E. F. CARRITT.

Aristotle's De Partibus Animalium. Critical and Literary Commentaries. INGEMARR DÜRING. Göteborg (Wettergren and Kerbers Förlag). 1943. Pp. 223.

MR. DÜRING's monograph could only receive the detailed examination it calls for in a journal devoted as MIND is not, to the study of classical philology, and from a reviewer more competent than myself to discuss the *minutiæ* of Aristotelian *Textkritik*. I must be content here briefly to describe its contents and to recommend it to the careful attention of students both of Aristotle's literary methods and of his biology. It is concerned partly with the problem of providing a basis for a really sound critical text of the *de partibus Animalium* by ascertaining the relations between the available MSS. and partly with a determination of the place of the work in the chronological sequence of Aristotle's " Works ", and the amount of revision which it has undergone before assuming its present shape. The following is a summary but I trust a fairly faithful one, of the author's conclusions. We can distinguish three stages in the growth of Aristotle's work in natural history and biology. (A) To the first stage belong *Historia Animalium* I-IV, *de Incessuo, de Motu Animalium*, all work that it is reasonable to hold was completed during the author's residence in the Troas and in Lesbos. (B) To a slightly later time belong *de Respiratione, de partibus Animalium* II-IV, *de Iuventute, de Vita et Morte, de Sensu* (first draft), *de Anima* (first draft) and also less certainly, *Meteorologica* IV, the whole constituting the original form of the great continuous ' course in biology '. There is reason to hold that the whole of this course in its first form was completed before Aristotle's removal from Lesbos to Pella. (C) Finally, and according to Mr. Düring, during Aristotle's

residence in Macedonia the course received additions and modifications. It now comprises *de partibus Animalium* I (written as a general introduction to the whole course), *Historia Animalium, de partibus Animalium* II-IV, *de Incessu, de Anima, de Motu Animalium, parva Naturalia, de Generatione Animalium.* The final arrangement of these writings under the titles and in the order exhibited in our Aristotelian *Corpus* is, of course, due to the editors who first published the whole in Rome in the first century B.C. Thus the main outstanding points of the author's articles are two. (1) The whole series of Aristotelian works dealing with animal life and biology belongs to the period of some twenty years between his removal from Athens at the death of Plato and his return as head of the Lyceum, and nearly all the material to the years of his residence in Assos and Lesbos. (2) In the *de partibus*, in particular, Book I can be shown to be later in composition than the rest of the work and to be meant as an introduction not merely to what we now know as the *de partibus* but to the whole biological πραγματεία.

It is not for me, who am neither a biologist nor a specialist in Aristotelian philology, to pronounce on these results, and I will only say that on the first point Mr. Düring is, of course, in general accord with the whole trend of recent work in the history of the Aristotelian text, and on the second, that as far as I can judge, he has at least fairly made out the posteriority of *de partibus* I to II-IV of the same work. Of his detailed criticism of the *textus receptus* and the conjectures of editors I can only say here that they seem to me always entitled to careful consideration, and, in my own opinion, for the little it is worth, often successful. If, as I should conjecture, the present publication is part of an intended edition of the whole work, I can only say that such an edition should be most valuable and welcome. Occasionally I note that Mr. Düring, who writes in English, has obviously been thinking in German, and that the reader will find it necessary to retranslate him into that language, as when, for example, he several times comments on the ' rash ' character of Aristotle's language, meaning only that the writing is " hurried " not that it is reckless or inconsiderate or when such a phrase as "it he than (= ' es sei denn ') in mutilated or maimed parts ", meaning " except perhaps in mutilated or maimed parts ". And I should ask him seriously to reconsider whether he should —in his notes on 643*a*, 1 and in one or two other places—have turned Aristotle's ἄτομον εἶδος into the ' concrete individual '. Does either Plato or Aristotle ever thus identify an *infima species* with an instance of it ? Is not that an unhistorical bit of Hegelianism ?

<div align="right">A. E. TAYLOR.</div>

Twentieth Century Philosophy : Living Schools of Thought. Edited by DAGOBERT D. RUNES. Philosophical Library Inc., New York, 1943. Pp. 571. $5.

ACCORDING to the jacket and, substantially, also according to a very brief preface—all the information that is given—the purpose of this book is to present " an overview of present-day philosophical trends by means of carefully chosen and elaborated essays which are broad enough to cover pretty well the entire field of modern philosophical investigation."

There are twenty-two essays ; and the book is divided into two parts. The nine essays of Part I deal with subjects, *e.g.* æsthetics, axiology ; the thirteen essays of Part II principally with movements, *e.g.* realism or

pragmatism. Two essays in Part I (Roscoe Pound on "Philosophy of Law" and Whitehead on "Philosophy of Life") and four in Part II (B. Russell on "Twentieth Century Philosophy", Santayana on "Transcendental Absolutism", W. P. Montague on "American Realism" and Dewey on "American Pragmatism") are reprints, presumably chosen on account of their peculiar appropriateness to this undertaking. Naturally some of these are among the best in the book, but since the more famous of them have already been sufficiently discussed, I shall not treat of them here.

Regarding the volume generally, the fairest question to ask is whether the book does give an adequate and comprehensive picture of contemporary philosophy. The answer is that it does. There is, for instance, no attempt to ascribe to contemporary philosophy a greater degree of unity than it possesses or any attempt to deny that many modern movements are content to follow their own aloof patterns. Again, the excellent essays on "Dialectical Materialism" and upon "Chinese Philosophy" (at the present hour, its traditions, its reactions to European thought and also to Buddhism) are very good evidence of a praiseworthy effort to avoid assuming that Western Europe and America constitute the entire intellectual globe.

Regarding the several essays in detail, it seems best to ask whether these essays are genuinely informative. Here I am afraid I have to offer a personal opinion, and would be rightly censurable if I pretended to offer anything more. In my opinion, then, most of the essays are skilfully informative, some of them very much so. A few do not seem to me to be particularly successful in this way.

As one would expect, Urban seems to me to write admirably on axiology, and Parker on Æsthetics. Tufts (who died while the book was in the press) could not be uninformative on ethics. Hall on Metaphysics, scenting a prevalent hostility to his enterprise, makes a careful, valuable and valiant effort to force a genuine action. More in the fashion, Feigl gives a studiously moderate account of Logical Empiricism and Farber, without making excessive claims, contrives to show how useful phenomenology may be.

Lenzen, a very practised writer on the philosophy of science, and D. C. Macintosh, equally practised in theology are, I think, rather less successful, though both their essays are well worth study. Dr. Flewelling, the indefatigable, gives yet another sober and plausible defence of "Personalism". A. C. Ewing is rather too short and, I think, rather unnecessarily dogmatic about Kantianism.

The essays which disappointed me, largely I am sure, through my own fault, were Boodin's on the Philosophy of History, Hoenigswald's on Hegelianism, and Maritain's on "The Humanism of St. Thomas Aquinas". In a rather staccato style, Boodin seems to me to pass far too rapidly from one questionable assumption to another. Hoenigswald, while giving a good deal of useful inside information, does not seem to me to tell a reasonably well-connected story. Maritain is eloquent but also tendencious and vague. He seems anxious to arrange for some sort of alliance between "Existenz-philosophers" and St. Thomas.

There are very good bibliographies, and the book, while not very suitable for continuous reading, handsomely rewards those who are inquisitive about particular patterns of philosophy, and have a mind to study these one at a time.

JOHN LAIRD.

Received also :—

J. Laird, *The Device of Government*, Cambridge University Press, 1944, pp. 173, 6s.

A. A. Luce, *Immaterializm* (British Academy Annual Philosophical Lecture, 1944), London, H. Milford, 1944, pp. 16, 2s.

A. C. Mukerji, *The Nature of Self* (Second Edition), Allahabad, The Indian Press, 1943, pp. xiii + 403, 7s. 8d.

R. A. Tsanoff, *The Moral Ideals of Our Civilization*, New York, E. P. Dutton & Co., Inc. : London, G. Allen & Unwin, 1944, pp. xix + 636, 30s.

B. C. Heyl, *New Bearings in Esthetics and Art Criticism*, New Haven, Yale Univ. Press ; London, H. Milford, 1944, pp. xii + 172, 16s. 6d·

L. Edelstein, *The Hippocratic Oath : Text, Translation and Interpretation*, Baltimore, Johns Hopkins Press, 1943, pp. vii + 64, $1.25.

P. A. Campbell, *Why the Universe ? or, Cosmopoictic Space*, San Francisco, George Fields, 1943, pp. 189, $2.50.

L. Freed, *Morality and Happiness*, London, Williams & Norgate, 1944, pp. 80, 5s.

K. B. Hathaway, *The Little Locksmith*, London, Faber & Faber, 1944, pp. 167, 8s. 6d.

Father James, *The Music of Life*, Cork, The Mercier Press, 1944, pp. 126 6s. 6d.

M. A. May, *A Social Psychology of War and Peace*, New Haven, Yale Univ. Press ; London, H. Milford, 1943, pp. viii + 281, 18s. 6d.

A. M. Meerloo, *Total War and the Human Mind*, London, G. Allen &Unwin, 1944, pp. 78, 5s.

The American Way : Selections from the Public Addresses and Papers of Franklin D. Roosevelt, ed. by D. D. Runes, New York, Philosophical Library, 1944, pp. 71, $1.50.

G. A. Wilson, *Reckoning with Life*, New Haven, Yale Univ. Press ; London, H. Milford, 1944, pp. x + 311, 18s. 6d.

Gilbert Murray, *Francis Macdonald Cornford, 1874-1943*, London, H. Milford, 1944, pp. 12, 2s.

H. J. Rose, *Robert Ranulph Marrett, 1866-1943*, London, H. Milford, 1944, pp. 16, 2s.

American Review of Soviet Medicine, Vol. I, No. 1, October 1943. Bimonthly. New York, American Soviet Medical Society. $6.00 per year ; $7.00 to foreign subscribers.

VII.—PHILOSOPHICAL PERIODICALS.

JOURNAL OF THE HISTORY OF IDEAS, iv, 1 (January, 1943). **Dana B. Durand.** *Tradition and Innovation in Fifteenth Century Italy.* [The first of two papers of a symposium whose " ultimate purpose is to test . . . the validity of the common opinion . . . that the Quattrocento marks a radical break with the Middle Ages and institutes the era of Modern Europe." Concludes that antecedents in scholastic tradition can be found for nearly all of Italian scientific achievements during this period, and that " the chief increments of knowledge—or units of innovation— must be classed as internal elaboration of traditional material, rather than as mutations."] **Hans Baron.** *Towards a More Positive Evaluation of the Fifteenth-Century Renaissance.* [The second contribution to the above-mentioned symposium. Maintains that although the Quattrocento did not develop outstanding novel works of science, it was a period of funda- mental cultural transformations which did influence the change from medieval to modern science during the sixteenth century. " . . . the importance of the Quattrocento lay in a new type of thinking, in a fresh approach to intellectual problems, and not in the extent of the innovations that were effected in the specific sciences and arts."] **Ernst Cassirer.** *Some Remarks on the Question of the Originality of the Renaissance.* **Francis R. Johnson.** *Preparation and Innovation in the Progress of Science.* **Paul Oskar Kristeller.** *The Place of Classical Humanism in Renais- sance Thought.* **Dean P. Lockwood.** *It is Time to Recognize a New " Modern Age."* **Lynn Thorndike.** *Renaissance or Prenaissance?* [These five brief papers comment on the issue raised by the afore-mentioned symposium. Prof. Cassirer concurs, on the whole, with Dr. Baron. " Mathematics had been an *element* in culture long before the Renaissance ; but in the Renaissance . . . it became a new cultural *force.* It is the inten- sity with which this new force fills the whole intellectual life and transforms it from within that we should regard as what is significantly new." Prof. Johnson comes to an analogous conclusion ; according to him, fifteenth- century Italy is an era in which " old material was being subjected to re- examination and elaboration, in which a closer co-operation between the scholar-scientist and the artist-craftsman was being evolved, and in which new political, economic, and social conditions were placing their impress upon scientific thought." Dr. Kristeller believes that by popularising the ancient classics, the fifteenth-century humanists " made an important, though indirect contribution to the development of science and philosophy." Prof. Thorndike alone breaks a lance for the Middle Ages. Not only were the thirteenth and fourteenth centuries more active and penetrating in natural science than was the Quattrocento, but the notion that the latter introduced into modern Europe the appreciation of natural beauty must also be abandoned. " For the Middle Ages loved variety ; the Renais- sance, uniformity."] **George Wittkowsky.** *Swift's MODEST PRO- POSAL : The Biography of an Early Georgian Pamphlet.* **Moses Hadas.** *From Nationalism to Cosmopolitanism in the Greco-Roman World.* **Louis B. Wright.** *The Noble Savage of Madagascar in 1640.* Recent Books, Periodicals, and Reprints Received.

iv, 2 (April, 1943). **J. Salwyn Schapiro.** *John Stuart Mill, Pioneer of Democratic Liberalism in England.* [An account of Mill's contributions to political and social theory. " Mill's importance in the history of the nineteenth century rests solely on his rôle as a pioneer of democratic

liberalism. . . . Mill's was the most potent intellectual influence that prepared the way for the advent of democratic liberalism in England."] **Heinrich Gomperz.** *Problems and Methods of Early Greek Science.* [Devoted to a general description and analysis of the intellectual procedures characteristic of the second half of the sixth century B.C. It considers the problems discussed, the methods of explanation used, and the methods of research employed. Four chief " thought-patterns " of explanation are noted : biological and political analogies, analogies drawn from the fields of artistic creativity, and mechanical analogies, each type being used in a characteristic way in connection with certain distinct problems. As for the methods of research employed, the paper concludes that " the need for increased factual knowledge and for testing assumptions by experience was hardly felt. . . . A scientific hypothesis was a . . . guess and the only criterion of its validity was its intrinsic plausibility."] **John Elof Boodin.** *The Discovery of Form.* [Sketch of the development of the notion of form and its attendant problems from the early Pythagorean conceptions, through Plato, Aristotle, and the medievals, down to the period of recent holistic philosophy.] **Bernard N. Schilling.** *The English Case Against Voltaire : 1789-1800.* [" . . . the condemnation of Voltaire as a prime mover of the French Revolution proceeds naturally from the prevailing English opinion that popular religious belief, especially Christianity, is absolutely essential to the maintenance of government and that anyone who weakens religious reverence opens the way to disorder and rebellion."] **J. Huizinga.** *History Changing Form.* [Discusses " historical form " in the sense of " something æsthetically arresting." Concludes that a history which has become " diffuse and indistinct " because of domination by the economic factor—as is alleged to be the case for American history from the Civil War to the appearance of Theodore Roosevelt—" may resume its dramatic tension and marked outline only when the political process regains a primary importance."] **Francesco Patrizi.** *De Spacio.* (Translated from the sixteenth-century text by Benjamin Brickman.) Books, Periodicals, and Reprints received.

PHILOSOPHY OF SCIENCE, ix, 4 (October, 1942). **R. S. Lillie.** *Living Systems and Non-Living Systems.* [" What I have emphasised is simply that the essential distinction between living and non-living systems consists in the special development of synthetic and integrative activity in the former class, and that this special peculiarity is to be correlated with their possession, in an enhanced or intensified form, of psychical as well as physical characteristics."] **G. Strömberg.** *Coherence in the Physical World.* **M. Black.** *Conventionalism in Geometry and the Interpretation of Necessary Statements.* [The author claims that no attempt to give a conventionalist analysis of necessary truth will derive support from Poincaré's conventionalism.] **F. E. Hartung.** *Operationalism : Idealism or Realism ?* [" As presented by some, operationalism in sociology is Kantian in its view of the universe," but as presented here " operationalism proceeds from philosophical realism."] **B. K. Bowdery.** *Usages of the Term ' Social '.* [" The word ' social ' is used in many different contexts, in each of which the word has a general root meaning, common to all contexts in which it is found. But in addition, there are other specific meanings, peculiar to each context . . ." . The purpose of this paper is to make explicit some of these more specific meanings.] **S. M. Thompson.** *Syllogistic Logic in Linear Notation.* [A system of notation is presented whose purpose is to simplify the application of the principles of syllogistic

logic to the analysis of arguments.] **W. Moore.** *The Indexical and Presentative Functions of Signs.* [Indexical and presentative signs, the author says, are what Prof. C. J. Ducasse calls " indicative " and " quiddative " symbols. An indicative symbol is " one of which the distinctive property is to orient our attention to some *place* ", whereas a quiddative symbol makes us conceive a certain *kind* of thing. The paper aims to show that this distinction is implicitly present in the writings of C. S. Peirce and that the distinction is " irreducible ".] Technical Scientific Section.

PROCEEDINGS OF THE ARISTOTELIAN SOCIETY. N.S.: Vol. xliii, 1942-43. 1943. **M. Ginsberg.** Presidential Address : ' The Individualist Basis of International Law and Morals '. [A useful paper, which first notes the kind of criticism to which the doctrines of individualism and natural rights have in recent times been subjected in the sphere of ethico-political theory, and then asks how far they are likely to undergo similar criticism and modification in the theory of international law and politics.] **J. Laird.** ' Hobbes on Aristotle's Politics '. [An account of Hobbes's criticism of the views of Aristotle in the *Politics* on the topics of human nature, law, and the body politic. Considering what Hobbes's own views are, one would hardly expect his views on Aristotle to be of much value, and they are not ; but if anyone wants to know what they are, he will find them stated in the paper.] **C. A. Mace.** ' Concerning Imagination '. [An interesting paper, of which the first part deals with the different meanings in which the term imagination may be used and the different species of imagination. The second part discusses a problem suggested apparently by Wisdom's articles in MIND, *viz.* what is it that happens when we imagine what is going on in someone else's mind ? The final form of the difficulty is, how can one imagine himself to be some one else, while continuing to be himself all the time ?] **H. D. Oakeley.** ' Is there a Higher Reason ? ' [The paper contains, first, statements about the views of Plato, Spinoza and Kant concerning the kinds of knowledge ; second, statements about the views of Whitehead and Collingwood regarding feeling, and of Jeans regarding recent science, and finally, statements about the writer's own views on some philosophical questions, as well as her own answer (the printer unkindly makes her call the answer a ' dusty ' one) to the question asked in the title. This wide variety of topics is dealt with in a space of 21 pp. ; there is little or no attempt at connected argument.] **C. Lewy.** ' Is the Notion of Disembodied Existence Self-contradictory ? ' [A paper which is clearly argued, but is in part not easy reading. In the first section it is argued in regard both to the pronoun I, and to any proper name for a person, that they neither ' stand for ' nor necessarily ' refer to ' a body. The writer therefore sums up as follows : " It seems to me that it is not a logically necessary condition for my existence that I should have a body, and that it is not a logically necessary condition for Smith's existence that Smith should have a body " (p. 72). The reader may be surprised, however, that no account is taken of psychological or psychophysical considerations. True, the title of the paper speaks only of self-contradiction ; also causal dependence is sharply distinguished from logical dependence and pronounced quite irrelevant (p. 64). But may not this sharp distinction assume a too abstract notion of personal existence ? The second part of the paper could not be easily summarized.] **H. F. Hallett.** ' World-Politics and the Philosopher '. [The paper takes in general the sensible line that in national and international politics what

is needed is neither an idealism which ignores the need of force to back it up, nor a realism which ignores moral ends, but a force that is under the control of moral ends. The writer apparently thinks it necessary to vindicate his reference in the title to ' the philosopher ' by introducing some ' philosophical ' assertions, couched in lofty and obscure phraseology, of which the following is a short specimen : " The divorce of right and might is the product of the ' original sin ' from which time emanates " (p. 104). " Statesmen ", we had been told, " must learn from philosophers ". Is this the kind of philosophy from which statesmen are to learn their business ?] **G. H. Langley.** ' Reason and Law '. [The law with which the paper is concerned is law in the political, not the scientific, sense. The general object of the paper is to bring out the vital relation between law (and legislation) and community life. The following quotation represents the general thesis of the paper fairly well : " Law has sprung from the need, inherent in persons, of co-operating for general welfare, and it represents something of the rational structure which human relations must have if they are to be fruitful in personal and social good " (p. 127).] **R. Jackson.** ' Kant's Distinction between Categorical and Hypothetical Imperatives '. [A long paper which does not justify its length. The writer seems to have nothing of any consequence to say about Kant's distinction, and, instead, wearies the reader with subtleties of his own invention, which seem unlikely to repay the labour of studying them.] **L. S. Stebbing, Sir J. H. Jeans, R. B. Braithwaite, E. T. Whittaker.** Symposium : ' The New Physics and Metaphysical Materialism '. [The question at issue in this symposium is whether the new physics gives less support to materialism than the old did. Miss Stebbing starts by showing that last century physics and biology did seem to point to a view of the world as being like a vast machine, in which human beings were mere helpless cogs. She then goes on to ask how the new physics alters this view, but she is confronted with the difficulty of understanding what is the view which the new physics, as represented by Jeans, substitutes for the old one, and the remainder of the paper is devoted to a statement of difficulties. The first part of Jean's paper, though short, is of much interest to the layman. It first sketches the relevant ' happenings in recent physics ', and then says, " The proper deduction was . . . not that continuity and causality had disappeared from the scheme of things . . . but that the universe is not the material structure that we had imagined " (p. 190). The second part of the paper contains a mixture of physics and philosophy which is more likely to perplex than to enlighten the reader. Braithwaite scores one or two points against Jeans, but gives no comprehensive criticism. Whittaker agrees with Jeans that the new physics does ' impinge on philosophy ', but he seems to be arguing from the paradoxical positions which Jeans is prepared to surrender.]

VIII.—NOTES.

L. SUSAN STEBBING, 1885-1943.

LIZZIE SUSAN STEBBING, M.A. (Cambridge), D.Lit. (London), was born in London near Wimbledon on 2nd of December, 1885. She was the youngest of the six children of Alfred Charles Stebbing, barrister, and Elizabeth Elstob, granddaughter of George Elstob, Earl of Durham. Her father died when she was two years old and her mother when she was sixteen and she was brought up under the charge of a guardian. She was a delicate child, not at first expected to live, and her education was spasmodic until she went to Girton College. Even then she was able to attend only a minimum of lectures. She wanted to read Classics, but it was felt that this would be too great a physical strain and she read History, taking Part I of the Tripos in 1906 and Part II in 1907. I think it was in her last term that she happened to open Bradley's *Appearance and Reality* one day in the library. She was instantly absorbed by it and determined to stay on to read Moral Sciences. Although unable to attend many lectures she took Part I of the Tripos in one year. She was coached by Mr. W. E. Johnson.

When she left Cambridge she set to work on a thesis *Pragmatism and French Voluntarism* for the London M.A. degree which she obtained in 1912. From 1913 to 1915 she lectured at King's College, London. In 1915 she, with her sister, the late Miss Helen Stebbing, and a Girton friend, Miss Hilda Gavin, and Miss Vivian Shepherd took over the Kingsley Lodge school for girls in Hampstead, and here she made her home—at first teaching history and always in closest contact with the policy and personnel of what became a large and flourishing school. In the same year, 1915, she became part-time Lecturer at Bedford College, London, and in 1920 full-time Lecturer, Reader in 1927 and Professor in 1933.

From the time she came to London her philosophical development owed much to the meetings of the Aristotelian Society. She met there Russell, Moore and Whitehead. It was under Whitehead's influence that she became interested in the philosophical foundations of science. But it was, I think, to Moore that she came to owe most.

Her earliest published work in philosophy was papers to the Aristotelian Society. She was president of the Society in 1933 and of the Mind Association in 1934. In 1931 she was visiting professor in the University of Columbia, U.S.A. The warm welcome she received on her visit to the States she always remembered with pleasure—and the view from the Empire State Building.

Her lectures were full of life. In discussion with her one could not expect to sit about in warm air—a stiffish breeze was usually blowing. But those who were given her vigorous teaching must, I think, have felt very great kindness and patience behind the sharp raps they were expected to stand up to in their training.

I always wished that she would write a book, or at least a paper, free from the pressure of other duties or any promise to have it done by a certain time. But no—there was always something, if not a committee meeting then a taxi for Ireland, and with a suitcase in her hand and a hat a trifle insecure upon her head she would be gone.

In 1931 *A Modern Introduction to Logic* came out. No other book of its kind had then been published I think. In it traditional logic is connected with what was then modern work in logic and metaphysics. Cohen and Nagel's *Logic* (1934) is simpler, much more readily grasped, and for the purpose of orderly description, without tears, of good arguments, better. But it does not bring out difficulties, on the contrary, it hurries past them and it does not pretend to make more than the slightest reference to metaphysical difficulties. Stebbing seriously introduces the metaphysical difficulties connected with the universe of discourse and knowledge by acquaintance and knowledge by description, induction, and the *a priori*. So does Mace in his *Logic*. His book is simpler than Stebbing's and he is readier to explore. But there is in Stebbing's something profoundly important, something the value of which has recently been more and more recognised—detailed examples presented seriously. Cohen and Nagel's is rich in examples. It is something about the way they are presented in Stebbing's which is so good. In her book students may easily get lost, but if they will exert themselves they may well find more than they would upon shorter, better marked tracks.

In MIND, numbers 164, 167, 168, 170 and 171 Mr. Joseph and Miss Stebbing dispute certain points in her book. Throughout her life she wrote for philosophical periodicals of all kinds, often at short notice, because if she wanted something cleared up she was " always ready to make a fool of herself " as she used to say.

Logical Positivism and Analysis was a lecture to the British Academy in 1933. It is in the main a discussion of the relations between Wittgenstein, Carnap, Schlick and Waismann on the one hand and Moore on the other.

Philosophy and the Physicists (1937) shows up the meretricious philosophical writings of Jeans and Eddington. But it does more than perform an intellectual service. This something more is there too in *Thinking to Some Purpose, Ideals and Illusions,* and in the essay *Moore's Influence* in *The Philosophy of G. E. Moore.*

Thinking to Some Purpose is a widely read critical study of certain confusions and tricks common in our thinking. It is excellently illustrated with real examples.

Ideals and Illusions seems to stress the importance for living of clear thought about ethical principles. Those of us who immediately want to insist that such thought may be as futile and dangerous for living as clear thought about æsthetic principles for art, should remember that Susan Stebbing also wrote,[1] on page 286 of *Philosophy and the Physicists*, " Our greed, our stupidity and lack of imagination, and apathy, these are the factors upon which the present sorry state of the world is largely consequent ", and " Our limitation is due not to ignorance, . . . but to the feebleness of our desires for good ". More knowledge of why we are like this is needed and calls for the courage and determination which Susan Stebbing showed in such a high degree.

In Susan Stebbing's writings, especially in the *Logic*, in *Thinking to Some Purpose*, in *Philosophy and the Physicists*, in *Ideals and Illusions*, and in the essay, *Moore's Influence*, I find something big and excellent which gives happiness. It isn't what is written about, still less is it a matter of style, though it is seen in both of these. It is something that could be seen, too, in her face, in the keen and honest eyes, thoughtful

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[1] Compare, too, her quotation from Locke on p. 475 of *A Modern Introduction to Logic.*

yet full of life. Sometimes when a child wants something very much we are tempted to rig the cards a little in his favour. Some children accept the deception, but others won't have it, and that not at all because they care the less. Most of us are obliged to compromise before the end in one way or another. Susan Stebbing had power to live and still to scorn dishonesty and still to care for what once was precious. However much she desired a thing to be so she would accept no assurance without evidence. She would smile, I think, if we pointed out how her own life provided one piece of evidence for her own proposition, " Yet despair need not be the last word. It lies within our power, if we so desire it, to make the familiar world we inhabit more worthy of habitation by beings who aspire to be rational and are capable of love." [1]

JOHN WISDOM.

R. F. ALFRED HOERNLÉ, 1880-1943.

REINHOLD FRIEDRICH ALFRED HOERNLÉ was born in Bonn. His very early childhood was spent with his parents in Calcutta, his later childhood with his maternal grandparents in Bonn. His schooling (1890-1899) was German, first in Gotha, later at Schul Pforta on the Saale. His university education (1899-1905) was entirely English, and at Oxford, where his parents settled. A Jenkyns exhibitioner at Balliol, he duly obtained his first in Greats and his John Locke Scholarship and proceeded to a senior demyship at Magdalen. His subsequent career as a professional philosopher was rich in geographical as well as in other variety. He was Lecturer in St. Andrews (1905-1908) in Bosanquet's department, Professor in what was then the South African College, Cape Town (1908-1912), Professor at Armstrong (now King's) College, Newcastle-on-Tyne (1912-1914), Assistant Professor at Harvard (1914-1920), again Professor at Newcastle (1920-1923), and thereafter, till his death, Professor in the University of the Witwatersrand, Johannesburg.

He regarded himself as what he really was, a liberal European *and* a British subject. On the technical point of nationality, his paternal grandfather, who was long resident in India, was a naturalised British subject, and his father, also resident in India, was a British subject. Hence young Hoernlé was correctly classified as an alien in the register at Schul Pforta (the Indian tradition was missionary, with eminence in Oriental languages). Technicalities apart, Hoernlé was German-English—hence the course of education his parents so deliberately arranged for him. He looked the part, and his speech revealed it as well as his mind. Indeed, although he spoke (as he wrote) in distinctive and often in distinguished English, being an orator when the occasion was suitable, no one could mistake him for an Englishman born and bred. The multi-continentalism of his career was in keeping with this, and also with the near-œcumenical outlook of a good European whose family traditions were more closely linked with England's expansion in distant places than with England herself. For such as he the bitter struggle between Britain and Germany in the present century was in some ways and on a short view a malign chance; but Hoernlé had a genius for friendship [2] and was never dismayed.

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[1] *Philosophy and the Physicists*, p. 286.

[2] " I think no one has ever come to the college for a brief stay who has been so much liked as Hoernlé " (from Prof. M. Phillips Mason, Bowdoin College, Brunswick, Maine).

He visited Germany between the wars, partly to see his mother (whom he took to his home in Johannesburg in 1939) but largely, also, to study the German mind, and, as the years went on, to understand the Nazism which he opposed so strenuously.

His collaboration with Bosanquet at St. Andrews strengthened his bent towards a synoptic philosophy. That is clear even in his first book, the *Studies in Contemporary Philosophy* (American-published in 1920), although it is a collection of essays. It is at least equally plain in his subsequent books on general philosophy, namely his *Matter, Life, Mind and God* (1923) and his books on *Idealism* (1925 and 1927). In the 1923 book, to choose that instance, there is an eloquent and very readable presentation (for the man was always a brilliant teacher [1] and expositor) of a vast theme treated briefly but neither slightly nor superficially, very sure of its ground, it is true, but, for all that, liberally receptive to many winds of doctrine, the newer the better and none the worse if their source was not particularly illustrious or very likely to become so. In his later years Hoernlé's publications on general philosophy were in the journals and not in books. This journal had its share, but many of these writings have to be sought in American periodicals not easily accessible in this country, for instance in *The Personalist* with which Hoernlé had rather special contacts as an occasional visiting Professor in Southern California. It is pleasant to learn from Mrs. Hoernlé's letter in MIND (April 1944) that many of these pieces are about to be published by the University of the Witwatersrand with a memoir by her. In such of them as I have seen, chiefly Platonic and Kantian, Hoernlé's capacity for combining stiffish metaphysics and rather minute scholarship with a pervasive and exciting enthusiasm for humanity, " as a meadow gale in June ", seems to me to have been very notable.

And there was much more than this. Like Bosanquet's, Hoernlé's social conscience was persistent, active and extraversive. Both the man and the philosopher in him were at the service of his fellows. As Newcastle knew so well—it still remembers his crowded popular audiences—Hoernlé could not think of himself as just an intra-mural teacher. In the Dominion of South Africa, Hoernlé, the liberal European, had the special stimulus of two great problems, the Afrikander-English problem and the European-native problem. A proof of his thoroughness as regards the first is that he speedily mastered Afrikaans and used it on principle in his extensive correspondence with Afrikanders and in his other useful and happy contacts with them. As regards the second, the native problem was Hoernlé's dominant interest in his Johannesburg years—at any rate up to the outbreak of the present war when Lt.-Col. Hoernlé started and organised an educational service for the South African Forces with a resolute and successful assiduity which cannot have delayed the thrombosis to which he succumbed. During the peace, however, it was the native question that engrossed him. He was not a " Kafferboetie and a negrophile " in the current South African sense of these words, but he did want to move the Europeans to understand the natives, and he was eager to help the natives to develop at a pace they could stand without losing their roots and their souls. His book, *South African Native Policy and the Liberal Spirit* (1939), was his fullest piece of writing on this subject, but he wrote a great many articles besides, and in any case was even more of a

[1] " He was an admirable teacher, one of the best we've ever had here," Prof. R. B. Perry writes me from Harvard.

committee-man than of a writer in this matter. For a time he was chairman of the South African Institute of Race Relations, and was long on its council ; he was chairman of the Bantu Men's Social Centre ; he was a delegate to the British Commonwealth Relations Conference at Sydney shortly before the present war. This list is illustrative and not exhaustive.

From the many South African tributes paid to his memory I select this : " Unless men and women who are now thirty years of age can show the persistent and undismayed devotion to the public good that Hoernlé did at sixty, our country's future is dark indeed."

<div style="text-align: right">JOHN LAIRD.</div>

L. S. STEBBING MEMORIAL FUND.

It is believed that many colleagues, pupils and other friends of the late Professor L. Susan Stebbing will concur in the desire that some permanent memorial should be instituted to her life and work. The suggestion is made that to this end funds be secured to endow a scholarship for postgraduate study in Philosophy. It is not proposed at the outset to define the conditions or possible places of tenure of such scholarship, but to draw up later a scheme for its administration.

Professor Stebbing cared deeply both for the advancement of philosophical studies and for the encouragement and aid of students. It is thought that the form of memorial put forward is one that she herself would have approved, and that it will be supported by all who admired her personality and her work in whatever field.

Contributions (the envelope marked " Stebbing Memorial Fund ") should be sent to the Hon. Secretary-Treasurer of the Fund, Professor Dorothy Tarrant, Bedford College, Regent's Park, London, N.W.1.

<div style="text-align: right">

C. D. BROAD.
G. JEBB.
C. A. MACE.
JOHN MACMURRAY.
G. E. MOORE.
H. H. PRICE.
HELEN M. WODEHOUSE.

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MIND ASSOCIATION : ANNUAL MEETING.

The Annual Meeting of the Mind Association will be held this year in New College, Oxford, at 3 p.m. on Friday, 7th July.

MIND ASSOCIATION.

Those who wish to join the Association should communicate with the Hon. Secretary, Mrs. Kneale, Lady Margaret Hall, Oxford; or with the Hon. Treasurer, Mr. H. Sturt, 55 Park Town, Oxford, to whom the yearly subscription of sixteen shillings should be paid. Cheques should be made payable to the Mind Association, Westminster Bank, Oxford. Members may pay a Life Composition of £16 instead of the annual subscription.

Members resident in U.S.A. may pay the subscription ($4) to the Hon. Assistant-Treasurer, Prof. B. Blanshard, Swarthmore College, Swarthmore, Pennsylvania.